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SCIENCE, LITERATURE, AND ART.

Observations on the Relations which exist between the Force, Construction, and Sailing Qualities of Ships of the Line.

WITH a former number of the *Journal of Science* *, we gave a brief disquisition on the *force* of ships-of-war; we shall now prosecute our inquiries, and endeavour to shew in what manner the force influences and connects itself with the theoretical construction and sea-going properties of such vessels.

In the construction of a ship-of-war, it becomes a matter of imperious necessity to consider the relation between its military and sailing qualities; for it is to very little purpose that the ship possesses the former, if it have not the latter to take them into action and to render them fully available and effective.

As the limits of a single memoir preclude us from entering into the proposed discussion relatively to all classes of ships-of-war, we shall at present confine our inquiries to those which are denominated *ships of the line*, reserving for a future paper a similar task in relation to frigates and smaller vessels.

The easy service of a piece of artillery, as we have already seen in our former paper, is dependent on its weight and calibre, the *maximum* of which, for sea service, we have therein endeavoured to indicate, from the examination of some facts with which we have been furnished from the practice of our own and foreign naval artillerists. But, however true this maxim is, with regard to a *single* gun, it requires to be modified when another gun is placed on each side; for the juxta-position occasioned thereby requires us to obtain such an interval between them, that the gunners, when at quarters, should not be crowded. When this precaution is neglected, much confusion and many accidents must arise from not having sufficient room to handle the tackling and various implements necessary to the service of a heavy piece of ship ordnance.

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* No. VII. of the New Series.

In land batteries it is the usual practice to allow a distance of 18 feet between the centres of the embrasures; but on board ship, wherein it is desirable to concentrate as much force as possible, it is common in the British navy to give no greater distance than 11½ feet between the centres of the lower ports of ships of the line. Now, when it is considered that the guns are frequently trained obliquely, whereby this space becomes much contracted, we may regard 11½ feet as the *least* distance that can be allowed from port centre to port centre of the heaviest tier of guns, in order that they may be efficiently served *.

Having this *datum*, we are enabled to determine what length of broadside is necessary to put a given number of guns into the lower battery of a ship of the line; and here we immediately perceive that the quantity of ordnance on the lower deck indicates the *minimum length* of the ship of whose armament it forms a part.

It may next be remarked that the guns also prescribe the *least* limit to the breadth of the ship; for, measuring from the hatches and ladder-ways on the lower-deck, a sufficient distance should be left between them and the rear of the gun-carriages, after recoil has taken place, for a clear communication fore and aft. The increased dimensions of modern ships have caused this limit to be much exceeded. The only part therefore of the half-breadth of the ship at the lower deck that has suffered any material increase in consequence, is that just specified, viz., the distance between the rear of the carriage after recoil, and the hatchway. In the old 80-gun ships on three decks, this space was not 6 feet*; in the present 84 it is about 9½ feet, and in the 120 about 10½ feet.

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* If this distance were somewhat increased, it would be highly advantageous, not only by giving more room for the gunners, but also by increasing the dimensions, and obtaining a better ship. Such a proceeding may be objected to on the score of economy; but we contend that the most *economical* ship is that which is the *most effective*.

Having thus seen how the force indicates the *least* limits of the simple dimensions of length and breadth, we have now to observe that the *depth* in ships of the line is a fixed dimension in comparison with the other two. Nevertheless, whilst the *minima* of the former are *directly* dependent upon, and indicated by the *force*, the *minimum* of the latter is likewise *indirectly* affected by the same element; for the space in the hold, which involves all three dimensions, must be sufficient for the stowage of the ballast, and the necessary quantity of water, provisions, and stores of every description, all which are ultimately referrible to the number and calibre of the ordnance. The *greatest* limit of the depth is pointed out by the draught of water in our roadsteads and ports.

As all the sailing qualities of a vessel are influenced in some way or other by the three principal dimensions and their mutual relations, we ought, after having determined on the *force*, to consider in the next place in what way it may be made to bring along with it an excellent ship. This naturally implies, that such an adjustment must be made of the ordnance as shall produce those relations between the dimensions which have been found conducive to the qualities essential to a ship-of-war, and particularly the velocity, the non-attainment of which more than counterbalances every other excellence. Instances are not wanting, even in the history of modern naval architecture, of a bad adjustment of the force being the ultimate cause of throwing *whole classes* of ships into disuse, by producing badly proportioned, slow sailing, and inefficient vessels. It is sufficient to mention the abolition of the 80-gun ships, on three, and 64's, on two decks, in corroboration of our assertion.

It must be confessed, that the connection between the principal dimensions of a ship-of-war and its force, has rarely, if ever, been understood, or duly appreciated in this country. Indeed, so little consequence has been attached to it, that even the ships composing the very recent experimental squadrons were ordered to be of a *certain tonnage;* thus at once fettering the efforts of the constructors, by a restriction which, of all others, is perhaps the most calculated to neutralize attempts at improvement. The present absurd method of estimating the tonnage of a vessel can only be tolerated on the plea of being a kind of standard by which ships may be bought and sold, and which, from long usage, being understood as such, it would perhaps be productive of some inconvenience to abolish; but that it should ever form a *datum* for the *construction* of a ship of war, cannot be too strongly condemned; because, from the manner in which it is obtained, there can be no permanent relation between it and the *real tonnage;* that is, *the difference between the weight of the hull and the total weight of the ship when fully equipped and ready for service.* To attempt the construction of a ship intended for the purposes of war, without referring the design to the quantity and calibre of artillery it is to bear, is a method of proceeding which, abandoning data at the very outset, involves the whole problem in distracting confusion, and inevitably concludes by leaving the solution to chance.

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* This deduction is made with a gun 9½ feet long, the common length for a 32-pounder.

When we attempt the construction of a ship of the line, it should be remembered that there are three ways of distributing the quantity of ordnance it is to be armed with;—

1st. The length of the ship being given, we may vary the number of decks on which guns are to be mounted.
2dly. The number of decks being restricted, we may augment or diminish the number of cannon carried on each.
3dly. The combination of the two foregoing methods also furnishes means, by which the force of a ship of the line may be adjusted, so as to suit the views of the constructor in other respects.

By adopting the first mentioned of these methods, we increase or diminish the height of the hull above water; and by resorting to the second, the length of the ship will be similarly affected. The third method we have noticed, will combine the effects of the two first, viz. an increase or decrease, both in the number of decks, and in the number of guns carried in each separate tier.

As the first method of adjustment can never be used without deteriorating all the most desirable properties of a ship, considered as a marine loco-motive machine, it is never resorted to: the same objections, however, do not exist against the second, nor to the third expedients when judiciously employed; and they afford ample latitude to the naval architect for making such a disposition of his force as shall produce the best sea-going qualities.

Since the introduction of cannon on board ship, towards the end of the fourteenth century, the progress of improvement has produced great changes in the disposition of the ordnance, and consequently in the relative proportions and sizes of vessels of war. From first carrying guns to fire *over* the gunwale, then through holes pierced in the breastwork of the weather-deck; the rivalry, which soon sprang up between the maritime powers of Europe, produced, at the beginning of the sixteenth century, ships of two entire tiers of ordnance*, but of very small dimensions, and in accordance with the then existing prejudices, having extravagantly lofty hulls. The celebrated Sovereign of the Seas, of 100-guns, built by Phineas Pett, in 1637, seems to have been the earliest ship of *three* whole tiers of cannon, that we have any distinct records of, and was at that time a great effort to combine force with a lofty hull. From the then very imperfect state of naval science, and a great, if not total absence of data, to guide its constructor, the attempt was a very hazardous one, and nothing but the bold increase of dimensions, which the science of Pett gave to this ship, rendered it at all successful†. The prepossession which, for such a length of time, existed in favour of these towering but inefficient ships, at length gave way to a more enlightened practice with all the maritime powers, excepting the Dutch‡. In this country the bold views of Pett died with him, and our naval architects relapsed into their old methods, from the perversity which usually attends ignorance, and caused the English navy not merely to stand still, as far as regarded the improvement of its ships, but absolutely to retrograde. They contented themselves with building lofty hulled vessels, which, from their small dimensions, were incapable of carrying their ordnance and stores without their lower ports being almost under water, besides being generally deficient in every requisite; and it was not until the middle of the last century that the superior qualities of the French and Spanish ships forced the constructors of our navy to begin a reluctant imitation of foreign models.

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* The contrivance of port-holes, by which a ship is rendered capable of using artillery between the gunwale and the water, took place in the year 1500, and is said to have proceeded from Descharges,—a French shipbuilder at Brest. The Harry Grace de Dieu, built in 1515, exhibits a very early adoption of this simple but important idea.

† Mr. Knowles, in some very able lectures on Naval Architecture, delivered in May, 1828, at the Royal Institution, tells us that the length of this ship was 167.75 feet, and breadth 48.33 feet, or about the length of the old Bellerophon of 74 guns, built in 1786, with a foot more breadth.

‡ It would appear from Pett's ships, that he had made some attempts towards suppressing the enormous and unnecessary weight of hull above water. Raleigh had, however, pointed out the necessity of doing this long before.

This humiliating proof of our inferiority, as naval constructors, was the natural consequence of the widely-different conduct of the governments of France and this country, in their treatment of science. Whilst the French Academy of Sciences, under the auspices of Louis XIV. and his successors, were holding out every incitement for men of genius and learning, to cultivate the mathematical principles of naval construction, the matter in this country was wholly neglected. The desire of that Monarch to obtain a maritime preponderance for his country, and the encouragement he wisely held out to men of science, may be said to have originated a new era in this noble art. The physico-mathematical laws of floating bodies, which, from the time of Archimedes till then, had lain dormant, began to be applied to ships; and the gradual developement of the fundamental principles of naval construction, by some of the most eminent mathematicians of the eighteenth century, gave rise to ships of the line of greatly enlarged dimensions, and superior qualities.

The many failures which took place, before and after Pett's time, in the construction of English ships of the line, proceeded from our naval architects not knowing how to appreciate, and contend with certain difficulties which so much top weight produces: difficulties, which, in a ship of more than two decks, rapidly increase. The superior knowledge of the French constructors enabled them at an early period of their efforts to triumph over these obstacles in their two-decked ships of the line; but they did not meet with equal success in their three-decked ships of 90 and 104 guns, although they were much superior to our own in qualities. The difficulty, indeed, of producing a first-rate ship of commensurate excellence appeared so insurmountable, that they were induced, after the peace of Utrecht, to discontinue their construction, and confine their ships of the line to those of two decks, mounting 74 and 84 guns. The constant patronage, however, given by the French government to its naval architects, amongst whom were many excellent mathematicians, was at length rewarded by seeing its fleets, in the war which broke out in 1778, again reinforced by three-decked ships of the line of superior force, and endowed with every desirable property, in almost as eminent a degree as those of two decks; and a further improvement in 1786, which increased the force to 120 guns, and carried the length to 209 feet, may be said to have completed the triumph of the French constructors. At this time, and until 1796, the largest three-decker in the English navy was the Victory, of 100 guns, and 186 feet long. In 1796, the Ville de Paris, carrying 110 guns, and 190 feet long, was built at Chatham; and the battle of St. Vincent, in 1797, put into our possession the San Josef, of 112 guns, which was, for many years, the largest and finest ship in the fleet.

The Spanish navy, at this period, possessed several ships of equal size and excellence with the San Josef. The almost uninterrupted alliance of Spain and France during the eighteenth century, and the consequent introduction of science into the Spanish dockyards, ultimately produced similar effects on the naval architecture of these two nations. One splendid proof* of the science of the French naval architects fell into our hands by the capitulation of Toulon in 1792, and gave rise to the increase of dimensions which has so much improved our three-decked ships of the line; but it was not till 1809, that the British navy could boast of the Caledonia, of 120 guns, and 205 feet long. But, although modern science has achieved such a victory, it cannot be said to have made any successful attempt beyond it as far regards the *number of decks*, which, two centuries ago, had reached its *present limits.* We cannot cite the instance that has occurred in the Spanish navy, of an efficient *four*-decked ship, in contradiction to this statement, because it had not the merit of being originally built as such†, and could not have undergone the alteration

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* The Commerce de Marseilles.

† The following description of this famous ship is given by James, in his Naval History, vol. ii. p. 66:—

"The Santissima Trinidada was built at Havana, in 1769, as a 112-gun from its previous state, as a three-decker, without detriment to its sailing qualities.

Thinking that it is very improbable that ships of the line, of more than three decks, will ever become common, if even attempted, through physical causes, we have to remark, that there remains now only one way of increasing the force of a ship of the line, the calibre of its artillery being given, and that is, by making use of the second adjustment of the ordnance we have mentioned. Actual trial has not yet pointed out where it ceases to produce improvement. The American line-of-battle ships of two decks are, for the most part, 206 feet long, and mount 102 guns of the calibre of forty-two and thirty-two pounds*: and they have ventured on a three-decker of at least 220 feet length, and corresponding force. The French are resuming, with vigour, the prosecution of maxims which originally emanated from them, and are said to be constructing a first-rate of 232 English feet in length†. These enlarged two-decked ships have been proved in actual service by the Americans, and found to answer the expectation formed of them. Whether the enlargement of the three-deckers exceeds propriety or not, remains to be determined. For our own parts we have very little, if any, doubt, as to the successful issue of this important and interesting experiment; and if a proper modification be made in the masting and canvas, there will not be any necessity for a size of masts and yards beyond what nature allows in sufficient plenty to our wants in this case.

Having endeavoured to shew the influence which the force of a ship of the line exercises over the principal dimensions,

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ship, similar to the San Josef, or Salvador del Mundo, except, probably, in possessing rather more breadth of beam. It appears that, some time between the commencement of 1793 and 1796, her quarter-deck and forecastle were formed into a whole deck, barricades built up along the gangways, and ports cut through them, so as to make the total number of 8-prs. on that deck equal, in amount, to the 12-prs. on the deck next below it. This accounts for 126 guns: the remaining four, we may suppose, were mounted on the poop. The Santissima Trinidada was, therefore, a flush four-decker that exceeded the three-decked 112's in force only by fourteen 8-prs., and four pieces of a still smaller calibre."

* The North Carolina, one of these ships, discharges at a broadside 1972 pounds of iron, whilst our Caledonia can only project 1568 pounds.

† A length of 232 feet would allow nineteen ports of a side on the lower battery.

we shall now proceed to inquire how far the *displacement* is affected by the same *datum.*

It is, or ought to be, an object of great solicitude, when the design of a ship-of-war is formed, to make its whole force available and effective, not merely in still water, but also when the sea is much agitated, and the power of the wind on the sails is such, as to produce a considerable inclination from the upright. Under the latter circumstances, if the lower ports be so depressed as to be below the waves, we lose the services of the lower battery in action, or at least fight its guns with very great inconvenience and liability to disaster. To prevent such bad consequences, it is absolutely necessary, in the construction, to give to the ports a due elevation above the assumed plane of floatation, and, with this plane as a base, to obtain a displacement, whose buoyancy shall be equal to the weight of the ship when ready for sea. If this precaution be not attended to in the first instance, the constructor must inevitably labour under all the anxieties attending a distracting uncertainty and a fearful looking for of failure in a vast and expensive structure. Every attempt at an after adjustment, in this particular, will produce not only a deterioration of some other important quality, but a general vitiation of the design; and nothing but a complete acquaintance with the *rationale* of his art can enable him to proceed with confidence and precision. It is true, that in the present state of science, the physico-mathematical laws of naval construction cannot all be derived *à priori;* but those which cannot, may be developed by the *analysis* of ships. The inductive method must naturally be disagreeable to those who are in the habit of mistaking self-confidence and fancies for real knowledge and well-founded deductions; but it is the only one capable, from its accumulation of data, of conducting us with certainty in the path of improvement; and, with the corps of naval engineers which this country now possesses, it is to be very much regretted that the energies of some of them are not, under *proper authority*, directed to the *permanent* prosecution of an undertaking so pregnant with advantage, as the analysis of the ships of the British navy. We have seen quite enough of the blind efforts of ignorant mystics and pretenders to be fully satisfied of the impotency of quackery, and to lament, that our leading naval authorities should be so much exposed to its deceptions, for want of a scientific reference of this description and its legitimate deductions.

A ship of the line to be fully effective in action, when the weather is rough, should not, in *any* case, have the lower portsills a less height above the water's *level* surface than 5 feet 9 inches, or 6 feet; and in one of *two* decks this height may be increased to 6½ or 7 feet without materially compromising other good qualities. There is, no doubt, some difficulty in effecting a greater height than 6 feet in ships of three decks, and they have already so much hull above water, that it becomes an object to prevent a further increase; but the same objection exists only in a far inferior degree in a ship of *two* decks; and as the failure of its lower battery would put it out of the line of battle, and, with the usual armament, would render it inferior in force to the heavy frigates of the present day, it becomes a matter of still graver importance to secure this class of ships of the line from losing the fire of their lower tier of ordnance.

The weight of the volume of water displaced by a ship when fully equipped for service, is susceptible of being divided into two principal parts, viz., the weight of the hull, and the weight of the equipment, in which we include the masting and ballast. The first-mentioned of these weights may be considerably varied from a difference in the practical carpentry, and in the specific gravity of the timber material used; but the second being that incidental to the armament, or such as is positively incurred by the adoption of a certain force, must bear a constant ratio, or nearly so, to that force, in ships of the same nation. What variation there may exist, must consist principally in the alteration of the stores and crew, from the war to the peace establishment, or from home to foreign service proportions. As, however, the good qualities of a ship-of-war are most especially valuable and desirable in belligerent times and on foreign stations, we should prefer constructing the best ship to meet such circumstances, and at the same time endeavour to preserve the *permanence* of its good qualities as much as the gradual consumption of water, provisions, and stores will allow.

The two following Tables will indicate very nearly the general state of naval construction in England, France, and Sweden. They give a comparative view of the force, and of some of the most important particulars which present themselves at the outset of a design for a ship of the line. We do not hesitate to say, that without a knowledge of these data, it is impossible to proceed with any degree of certainty, or to possess any control over the results of our operations. It is proper to observe, that the elements of the English ships given in the second table, have been calculated from the draughts of water and height of lower port-sills given in official returns. The 120-gun ship, here partly analysed, is of the Caledonia class; the 84, of the Canopus, or French Franklin, class; the ship of 76 guns is the Bulwark; and that of 74 is the Blenheim or Ajax, similar to which about 40 ships have been built. The elements of the French ships are mostly derived from the *Ordonnance de la Marine* of 1786; and those of the Swedish from the superb work of Chapman, published in 1806 *. The weights and measures have been carefully reduced to those of England †.

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* It is much to be lamented that this work of Chapman is, as it were, a sealed book to the English constructor, from its scarcity, great expense, and being in a foreign language very little known in this country. It is replete with valuable data and information with regard to ships-of-war, and it would redound greatly to the honour of our Admiralty to have it promulged in our dockyards in an English dress, and to order it to be used as a text-book in our Naval Architectural School at Portsmouth, which, it must be confessed, is at present much in want of such a well arranged and useful treatise.

† It will be perceived from the first table, that Chapman tasks the manual power of Swedish seamen at a much higher rate than even the French service does, and he says unequivocally, that their strength is fully equivalent to it. We presume, therefore, that he reckons on the superiority of *his* gun-carriage for facilitating its service. In our former paper we have given an exposition of our views with regard to increasing the powers of our sea service ordnance, and have detected the fallacy of the arguments on which our maximum calibre, the 42-pr., was discarded. We now feel additional confidence in the truth of what we have before stated as to the propriety of restoring that powerful gun to our navy, not only from what Chapman has advanced, but also from having before us Commander Marshall's very recent work, developing a system of mounting naval ordnance, at once novel, simple and efficacious. No one, we presume, can deny its originality; and actual trial at sea for the last two or three years has fully determined its efficacy. We shall at an early opportunity endeavour to take a detailed view of Commander Marshall's principle, which is calculated to produce the most important ameliorations in a system of mounting sea service guns, that has continued from the times of the Tudors to the present day with scarcely an improvement.

TABLE I., *exhibiting the Comparative Force of English, French, and Swedish Ships of the Line.*

Navy belonging to	Total Number of Guns.	LOWER DECK.			MIDDLE DECK.			UPPER DECK.			QUARTER-DECK AND FORECASTLE.			Weight projected at one broadside, in pounds avoir.
		Number of Guns.	Calibre.	Calibre, in pounds avoir.	Number of Guns.	Calibre.	Calibre, in pounds avoir.	Number of Guns.	Calibre.	Calibre, in pounds avoir.	Number of Guns.	Calibre.	Calibre, in pounds avoir.	
English	120	32	32	32.00	34	24	24.00	34 Congreve.	24	24	8 12 Car.	12 32	12.00 32.00	1568.00
French.....	120	32	36	38.86	34	24	25.92	34	12	12.96	8 12 Car.	8 36	8.64 38.86	1770.76
Swedish ...	110	30	48	55.58	32	42	48.62	30	24	27.73	18	12	13.84	2152.13
English	84	28 2	32 68 Car.	32.00 68.00	...	...	....	32	24	24.00	8 Congreve. 14 Car.	24 32	24.00 32.00	1220.00
French.....	80	30	36	38.86	...	...	... .	32	24	25.92	6 12 Car.	12 36	12.96 38.86	1269.66*
Swedish ...	80	30	42	48.62	...	...	....	32	24	27.73	18	12	13.84	1297.54†
English	76	28	32	32.00	...	...	....	30	24	24.00	6 12 Car.	12 32	32.00 12.00	1018.00
English	74	28	32	32.00	...	...	....	28	18	18.00	6 12 Car.	12 32	12.00 32.00	928.00
French.....	74	28	36	38.86	...	...	....	30	18	19.43	6 10 Car.	8 36	8.64 38.86	1055.76
Swedish ...	74	28	36	41.69	...	...	....	30	24	27.73	16	12	13.84	1110.33

* If this ship were mounted with two more 12-pounders, and two more 36-pounder carronades, the weight of the broadside would be increased to 1320.82 pounds avoir.

† If the armament of this ship were augmented by four more 12-pounders, the weight of the broadside would be increased to 1324.68 pounds avoir.

The weight of the 48-pounder proposed by Chapman is 84·36 cwt., or 170 times the weight of the shot; and is 9 feet 9 inches long, English measure.

The weight of his 42-pounder is . . . 74·23 ,, 171 ,, ,,
and is 9 feet 4½ inches, English, long.

The weight of the common 36-pounder, Swedish, is 65·52 ,, 176 ,, ,,
and is 9 feet, English, long . . .

Ditto, Ditto, 30-pounder, ditto, is 55·42 ,, 178 ,, ,,
and is 8 feet 7 inches, English, long . .

Ditto, Ditto, 24-pounder, ditto, is 45·17 ,, 182 ,, ,,
and is 7 feet 9 inches, English, long . .

The weight of the French 36-pounder, is . . 71·00 ,, 207 ,, ,,
and is 10 feet 1½ inches, English, long . .

Ditto, Ditto 24-pounder, is . . 52·00 ,, 202 ,, ,,
and is 9 feet 7 inches, English, long . .

In our former paper the Swedish 36-pound shot was erroneously stated at being equivalent to 33·73 pounds avoirdupois. This mistake arose from regarding the calibre as expressed in common Swedish pounds, instead of the pounds in which the Swedes express the calibre of their guns.

Table II., *giving a Comparative View of some of the primary Elements of Naval Construction derived from English, French, and Swedish Ships of the Line, when equipped and stored for foreign service, in time of war.*

Of what construction.	No. of Guns.	No. of Crew.	No. of Months.		Length in feet to outside of wales at the water section's middle line.	Breadth in feet to outside of the wales at the water section.	Draft of water in feet.		Height of Midship lower port above the water, in feet.	Tons per inch submersion, with water section as a base.	Displacement in tons.	Tons displacement per gun.	Weight of hull, in tons.	Real tonnage.	Real tonnage per gun.	Ballast, in tons.	Real tonnage, exclusive of ballast.	Real tonnage. per gun. exclusive of ballast.
			Water	Provisions.			Forward.	Abaft.										
English ..	120	875	3	6	205	54·41	25·02	26·77	4·61	23·50	4841	40·34	2532	2309	19·24	326	1983	16·52
French ...	120	1098	3	6	209	55·32	24·74	27·17	5·78*	25·00*	5010*	41·75	2327	2683	22·35	622	2061	17·17
Swedish ..	110	1000	2·7	5·4	204	56·34	22·96	24·91	6·31	24·05	4251	38·85	1828	2423	22·23	483	1940	17·63
English...	84	700	3	6	196	52·41	23·20	25·78	5·30	20·50	3806	45·31	1915	1891	22·51	240	1651	19·65
French ...	80	839	3	6	194	52·41	22·82	24·42	5·86	20·00	3687	46·09	1651	2036	25·45	489	1547	19·34
Swedish ..	80	706	3	6	183	51·09	20·85	22·64	6·74	19·43	2986	36·59	1469	1517	18·22	296	1221	15·26
English...	76	650	3	6	182	49·75	22·30	23·30	4·89	18·73	3322	43·71	1838	1484	19·52	220	1264	16·63
English...	74	600	3	6	176	47·90	22·05	24·10	5.16	17·63	3060	41·35	1690	1370	18·51	174	1196	16·16
French ...	74	690	3	6	183	48·77	21·14	22·92	5.86	18·29	2938	39·71	1307	1631	22·04	331	1300	17·54
Swedish ..	74	658	3	6	177	49·71	20·21	21·99	6·65	18·23	2681	36·43	1368	1313	17·90	256	1057	14·28

* These Elements have been obtained by calculations on the draft of the Commerce de Marseilles.

From the last Table may be derived the following one, in which these ships are, with respect to the height of lower ports above water and the quantity of displacement, referred to the same standard of comparison, viz., the real tonnage of the English ships of the same number of guns; or, in other words, the whole of the foreign vessels are supposed to be equipped for foreign service, in time of war, as if they belonged to the English navy; ten guns more being mounted in the Swedish three-decker, and four in the French and Swedish 80-gun ships.

TABLE III.

Of what Construction.	No. of Guns.	No. of months.		Height of Midship Port above water, in feet.	Displacement, in tons.	Displacement per Gun, in tons	Real tonnage.
		Water.	Provisions.				
English	120	3	6	4·61	4841	40·34	2309
French	120	3	6	7·02	4636	38·63	
Swedish	120	3	6	6·68	4137	34·47	
English.....	84	3	6	5·30	3806	45·31	1891
French	84	3	6	6·45	3542	42·16	
Swedish....	84	3	6	5·03	3360	40·00	
English	74	3	6	5·14	3060	41·35	1370
French	74	3	6	7·01	2677	36·17	
Swedish	74	3	6	6·38	2738	37·00	

(*To be continued.*)

On Siliceous Gravel. By Robert Venables, M. B., St. Mary Hall, Oxford.

Some foreign writers speak of urinary concretions consisting of *siliceous matter*, in sufficient quantity to establish the character of the species; and Berzelius, in his treatise on the blow-pipe, gives a formula for their discrimination*. He even points out the characters distinguishing this class from those of the phosphate of lime†. From these circumstances, it might probably be inferred, that siliceous deposits from the urine were by no means of very rare occurrence. It does not, however, appear that any of the British practitioners, who have been most conversant with urinary disorders, have observed them either as gravel, or as forming any sensible portion of urinary concretions. Dr. Marcet has not mentioned its existence in any one of the many calculi which he analyzed, and if it had existed it could not have escaped so close and so accurate an observer. Dr. Wollaston, I believe, has not met with it either; and Mr. Brande, in his Manual, does not speak of it as constituting any portion of urinary calculi. Of 328 calculi—part of the collection of the Norwich Hospital—analyzed by Dr. Yellowley‡, there is not one stated to contain the smallest proportion of siliceous matter. Hence then, it would appear, that in this country, at least, urinary siliceous concretions are extremely rare, and that, as yet, there has not been an instance recorded§.

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* "Alone," he says, "they leave an infusible scoriaceous ash, which fuses with a small quantity of soda, slowly, and with effervescence, into a more or less transparent glass globule." Berzelius on the Blow-pipe, translated by Children, p. 332.

† "A proof that these calculi (phosphate of lime) are not siliceous, is, that they swell up with soda without vitrifying, and when dissolved in boracic acid, and then fused with a little iron, they give a regulus of phosphuret of iron." Ibid., pp. 330, 331.

‡ Philosophical Transactions, 1829, part 1st.

§ Of 600 calculi, analyzed with great care and exactness, by Fourcroy and Vauquelin, only two were found to contain silica. Professor Wurzer states, that he detected silica to the amount of one per cent. in a calculus analyzed by him. Its composition he states to have been as follows:—

Lithic acid . . .	75·34
Phosphate of lime . .	17·33
Animal matter . .	6·33
Silica	1·00
	100·00

On the contrary, the highest authority on the subject in this country, is decidedly against the opinion that silex does form any part of urinary gravel. Thus Dr. Prout, in his very valuable Treatise on Urinary Disorders, observes, "Silex has been stated to constitute urinary sediments, and even to form a part of urinary calculi in some instances; but this assertion requires to be better authenticated than it is at present, before it can deserve credit*." Indeed, Dr. Prout communicated to me, personally, his doubts as to the existence of silex, either as gravel or calculus, and expressed his desire of having the point satisfactorily decided. Certainly Berzelius asserts, that it ordinarily exists in the urine, in minute quantity, and that it is probably derived from the water which we drink; but the foregoing facts tend to prove, that in this country, at least, silex is not apt to appear either as gravel or in the form of urinary calculi.

I have, however, met with two instances, and which, as affording matter interesting no less in a scientific than in a professional point of view, I deem of sufficient moment to claim attention. From the nature of the facts and the questions which they involve, I have preferred the *Quarterly Journal of Science* as the most eligible medium of communication.

The first case occurred in the practice of my friend, Mr. Henry Bird, surgeon of this town†, and was that of a woman thirty-two years of age, married, having two children living, rather unhealthy, the disordered tendencies being to pulmonic irritation. She has been pregnant, in all, seven times, including two miscarriages; of the five children born alive, three died and two only now survive. The unfortunate sufferer herself presents every indication of bad health and a broken-down constitution—emaciation, languid, anxious countenance, hectic flush, foul sulcated tongue, hot feverish state of skin, though, at times, subject to perspirations, frequent wiry pulse,

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Allemain, an Italian apothecary, states that he found silica in excessive proportion—20 per cent.—in a calculus which he analyzed; but as he has given no detail of his mode of analysis, some doubts may be entertained as to the correctness of the results.—*Vid. Ann. de Chimie*, xxxii. p. 221, and xlv. p. 222, and *Gehlen. Journ.*, second series, ii. p. 265.

* Prout, on Calculus, &c. pp. 28, 29, second edition.

† Chelmsford, Essex.

with dyspnœa. When she consulted Mr. Bird, she complained most of the pulmonary affection; but after the urgency of the symptoms had been subdued by venesection and other appropriate means, she was seized with severe pains in the back and loins. They were of a sharp lancinating description, and shot, as she termed it, from the kidney to the bladder, observing the exact course of the ureter. These pains continuing to increase, and becoming every day more distressing, Mr. Bird requested me to see her, which I did on the 20th May, 1828. I found her in the state above described, and with other symptoms of severe urinary disorder. She told me that she had for a long time considered herself subject to gravel. A few days after I visited her, she gave to Mr. Bird two small concretions passed at separate times, and with a short interval between. Mr. Bird handed me one, but the other he unfortunately lost, and he had not attended to its characters. That which I received presented the following characters:—It was about the size of a large pin-head, rather opaque, of a whitish colour, and of an oblong shape, slightly excavated. The shape very much resembled that of a cystic oxide calculus in my possession, and which was passed by a woman in this neighbourhood; the details of which I shall endeavour to communicate in a future Number. This figure I consider as bearing some slight resemblance to that of the kidney itself. Exposed to the flame of the blowpipe, it underwent no change whatever, although it was urged for upwards of ten minutes, and exposed during this time to the strongest heat I could excite with this instrument. It scratched glass like common flint. Neither acetic, nitric, nor muriatic acids, though boiled upon it, produced any effect. It was then treated with caustic potass, but without any sensible effect. Having occasion to go to London, I took it and shewed it to my friend Dr. Prout: he, at first, thought it was a tooth, but, on looking at it more closely, he immediately pronounced it silex, after writing with it upon one of the panes of glass in the window of his study. I then detailed to Dr. Prout its history, and it was upon this occasion that he told me his doubts as to the existence of urinary siliceous concretions, and expressed his desire of having the question satisfactorily and unequivocally decided. Lastly, on my return to

Chelmsford, I again submitted it, under a less objectionable form, to the action of the re-agents already mentioned and with similar results. I then mixed it with soda, and exposed it to the flame of the blowpipe, when it melted slowly, and at last fused into a semi-vitreous globule, with scarcely any degree of transparency. These refractory characters place the nature of this substance beyond any doubt. Being in the habit for the last fourteen or fifteen years of examining the general properties of the urine in all cases, where practicable, in which I am concerned, and having never met with any similar production, I felt doubtful as to the urinary origin of this substance. I have not the slightest suspicion of any attempt at imposition * on the part of this poor woman; but the siliceous substance might have been in the utensil, and the patient seeing it, after having voided the urine, might have imagined that it passed from the bladder with this fluid. This makes the loss of the other little calculus, which was passed at a day or two's interval, the more to be regretted, as its nature would have tended, in some degree, to clear up the mystery. I was not, therefore, inclined to attach much importance to this single instance, more especially as its urinary origin was not unequivocally proved. But having lately met with another instance in which siliceous matter, in the form of very minute angular grains, has been passed with the urine, I can no longer doubt the circumstance, and have therefore determined to present the facts to the scientific part of the profession.

The patient, in this instance, is also a married woman, thirty-four years of age, of very delicate constitution, and, in general, bad health. She has also, for a long time †, suffered with symptoms of urinary derangement, and has frequently passed blood with the urine. There is also a considerable degree of uterine disease, as, although she has been several times pregnant, she has never given birth to a living child, nor gone her full time; yet her mother has had fifteen

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* Dr. Marcet states, that patients frequently attempt to impose upon medical men, without any ostensible motive. Could he have met with instances of the above description, and considered them as attempts to deceive and impose upon the practitioner?

† She states since 1819.

children. She first consulted me on the 22d of October, 1829, complaining of very severe and excruciating pains in the back and loins. The pain also passes along the course of the ureter from the kidney to the bladder. She now, for the first time, perceived a reddish-looking sand at the bottom of the vessel (a tea-cup), in which the water was made. The urine was extremely scanty, not making much above half a tea-cup full at several times. She brought the specimen with her, and on looking at the bottom of the cup, I saw a very small quantity (about ·8 of a grain) of a reddish-looking sand, which I took for lithic acid, the minute crystals of which it very closely resembled. Upon this conviction I prescribed for her, but desiring her to leave the vessel with its contents, till a convenient opportunity for examining them. The urine had been passed in the morning about an hour or two before it was brought to me, and, in the course of the day, I commenced the examination. Having poured the urine off, the sand was found collected together in a small mass at the bottom of the cup. A small quantity being placed upon charcoal, and urged with the blowpipe, presented a scoriaceous appearance, without consuming or leaving the white ash usually left by lithic acid. Reddened litmus paper, being moistened and brought in contact with it, indicated no alkaline re-agency. A small quantity was now placed in a small glass capsule, and a drop of nitric acid being placed upon it, it was heated. An effervescence took place, but on continuing the heat till the acid was dissipated, the dried mass presented nearly the same appearance as before the experiment. On being exposed to the vapour of ammonia, there was a slight indication of the presence of lithic acid interspersed through the mass. I now examined it with a small magnifying glass, and perceived that it consisted of small angular crystals, transparent, some colourless like quartz, others of an ambery or topaz colour, and having a certain degree of refractive density. On treating them with caustic potass *, and then washing them, nearly the

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* Silica is soluble in the caustic fixed alkalies ; but it is not very sensibly affected by the liq. potassæ of the shops, unless the silica be in a state of minute mechanical division, and after a prolonged digestion. The lithic acid, on the contrary, is very speedily taken up by this

whole of the red colour was removed, and the mass became nearly colourless. They resembled the very fine crystals which we frequently see in sand or gravel upon the sea-side. On taking a small crystal, and pressing it with my nail, and drawing it over a piece of window glass, it scratched it like common flint. Mixed with soda, and heated with the blowpipe upon charcoal, they melted slowly, and with effervescence, into a vitreous globule. In very small quantity, they gave a perfectly transparent globule of an amber or orange colour; in larger quantity, the vitrification was less perfect, and the globule more opaque. These characters perfectly agree with those of silex.

I have since had several opportunities of attending to the particulars of this case; and I have satisfied myself, beyond the possibility of doubt, that this siliceous sand is passed with the urine. On the first examination, I thought it possible that the sand might have been in the cup, and the urine have been voided upon it; but, upon attending to all the circumstances, it is impossible to admit of such a solution. In the first place, the red colour was owing to a thin coating of the alkaline lithates, ammonia, and lime. Thus, by treating them with

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menstruum. Hence, no mistake could arise from the hasty exposure to the action of liq. potassæ, to which the sand was subjected. Nothing but the lithic acid coating was dissolved. This was satisfactorily proved by the addition of distilled vinegar, the separating and washing the precipitate, and re-dissolving it in nitric acid, which was attended with the usual appearances. On evaporation to dryness, the carmine red colour afforded sufficient proof of the presence of lithic acid, which was still further confirmed by the re-agency of the vapour of ammonia.

However, having treated a small quantity of the sand with nitric, muriatic acid, and caustic potass in succession, and washing after exposure to each re-agency, the residue being pulverised in an agate mortar, and reduced to a state of extreme comminution, was digested in potass and dissolved. The solution being supersaturated with muriatic acid, the silica was precipitated, and reduced by evaporation to a gelatinous mass. It was then evaporated to dryness, and was tried with warm distilled water by means of "Wollaston's Fountain of Compression," till the washings ceased to give a precipitate with nitrate of silver; the residue, being mixed with soda, fused into a transparent vitreous amber or orange-coloured globule. Quartz, treated in the same way, afforded precisely a similar result. The quantity of sand that could be obtained from all the specimens of urine was not sufficient to institute experiments upon a more extended scale without sacrificing the whole product, which I was unwilling to do, as I wished to preserve a small portion as a record of the most important feature in this interesting case.

acetic or muriatic acid the lime was dissolved, leaving the lithic acid behind. On pouring off the acid, and adding oxalate of ammonia, oxalate of lime precipitated, and was recognised by its peculiar characters. The residual mass being treated with potass, the lithic acid was dissolved and taken up. Pouring off the solution, and supersaturating with vinegar, the lithic acid was precipitated in the form of a whitish powder.

The fact of these grains being covered with a coating of known urinary origin, fully proves that they must have been in contact with the urine sufficiently long to have had this coating deposited upon them. This could not have arisen from their accidental contact with the urine in the cup, because the urine was not acid, but neutral or rather alkalescent, as will appear when we consider the properties of this fluid. But further, they were not exposed to the action of the urine, after being voided sufficiently long to have acquired this coating. Therefore, it must have been deposited or secreted upon them in the kidney, or in some other part of the urinary organs. But in order to place the question beyond dispute, I washed the cup myself, and had the urine passed into it under circumstances which could not admit of deception, and I found the same sand in the urine voided as above-stated. Hence, there cannot be a doubt of their being passed with the urine from the bladder. I have had several specimens brought to me, and they all contain more or less of this siliceous matter.

A detail of the treatment in these cases would be inconsistent with the object of the present communication, which is the establishment of certain pathological facts, which have been doubted, and upon which the authority hitherto has been questionable. The morbid condition of the urine, however, is a necessary preliminary to the future remarks, and, indeed, essential to my purpose. I shall, therefore, proceed to this detail in each case.

In the first case, I received two specimens on the 27th of May, 1828;—the one passed after dinner the preceding day, was turbid from the mechanical suspension of a considerable quantity of muco-purulent matter, mixed with the alkaline lithates. On standing for a considerable time these subsided, leaving a transparent amber-coloured urine floating above.

It reddened litmus paper, had a specific gravity of 1.026, and contained a considerable excess of urea.

The second specimen passed the following morning before breakfast, looked clear and almost like spring water; had scarcely any smell or taste; sp. gr. 1·02, faintly reddened litmus paper, and contained a very slight excess of urea. After standing, a greasy-looking crystalline film floated on the surface, which consisted, principally, of the triple phosphate, with a little muco-purulent matter. They were both serous.

I examined this woman's urine again on the 27th of August, 1829. It was turbid, opalescent, from the suspension of a large proportion of muco-purulent matter with the lithates of ammonia and lime, and which were of a whitish-yellow colour. There was also a small quantity of the fusible phosphates. It faintly reddened litmus, and the sp. gr. was 1·025. There was a great excess of urea, and it was serous. This unfortunate woman has very bad health, is very frequently attacked with severe illness, and has lately voided blood with the urine. She has not, so far as I know, passed any more gravel or calculi.

The second case, the general symptoms of which have been already stated, and, in many points, are analogous to those of the first, consulted me on the 22d of October, 1829.

The specimen of urine which she brought was turbid and opalescent. On being set aside, it became clear and transparent by the slow subsidence of purulent matter, mixed with the alkaline lithates, of a pale red colour. It was neutral or rather alkalescent, of a deep amber colour, serous, sp. gr. 1·020; no excess of urea; siliceous sand at the bottom of the cup.

28th. Siliceous deposit in very small quantity, but sufficient to distinguish it. The urine turbid from the muco-purulent matter, which, subsiding, the urine became clear, transparent, and of an orange colour. It was neutral, serous, sp. gr. 1.022; no excess of urea.

29th. Urine turbid as before, and exhibits the same general properties, reddens litmus, and contains rather more lithic acid; serous, sp. gr. 1.024; slight excess of urea: quantity of siliceous sand increased.

30th. Urine turbid, opalescent, and wheyish; a small quantity of the siliceous sand at the bottom of the cup. On being left at rest, a quantity of whitish-looking powder subsided, leaving the supernatant urine clear, transparent, and nearly colourless. This urine was alkaline *, and the deposit proved to be the lithates of soda, ammonia, and lime, intermixed with a large proportion of fibreo-albuminous matter, such as I have seen frequently discharged from the kidneys, in diseases of these organs, attended with a serous condition of the urine. This urine contained albuminous matter. In other respects its properties nearly as those of the preceding specimens.

31st. She states she has not passed any appreciable quantity of the red sand since yesterday. The urine more plentiful, and she feels much relieved since passing the sand above described.

It would be useless to enter more at length upon the general history of these cases, as, with very little differences, perhaps of an accidental nature, what has been already stated comprises the principal circumstances, so far as they have come to my knowledge.

In reviewing the above facts, two or three interesting questions suggest themselves. Was this gravel the product of diseased action in the kidneys, and secreted as the oxalate of lime, cystic oxide, or any other morbid and unnatural product; or was it introduced into the stomach with the patient's drink? I shall consider the fact of this gravel coming from the bladder, as fully established; for none who will give me credit for veracity in the above detail, can for a moment question the renal origin of the sand, and its passage from the bladder with the urine. If it be a secretion by the kidneys, we naturally inquire, is it the result of morbid function without any supply of the materials,—as by the use of siliceous waters or other siliceous substances,—or does the morbid operation consist in the mere separation and aggregation of the siliceous matters in the water, or aliments ordinarily consumed by the patient? The spring waters in general use in this neighbourhood are hard, and contain lime and siliceous matters in solution; and

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* She had been taking the liquor ammon. acet.

indeed there is a strong tendency to urinary affections, partly in consequence of such a locality. This patient, however, is a native of Gloucestershire, and has been resident here only about six weeks; and during this time has daily used the common pump water of the place. She also states that her grandmother obliged her, when young, to drink a great deal of sea-water, and she thought the sand thus introduced might now be appearing; but as many years have since elapsed, we can hardly look to so remote an origin. There is one important circumstance in her history which I must not omit, that urinary disorders prevail in her family.

It occurred to me that there might be a possibility of a quantity of this very fine siliceous sand being pumped up with the water, and remaining in mechanical suspension, as I have occasionally seen. In this way, perhaps, some might be disposed to account for its appearance in the urine; but the objections to such a solution are many and various. In the first place, others should be equally liable under similar circumstances; yet, notwithstanding I have been in the habit of attending closely to the properties of the urine, I have never met with any instance of a similar nature, and it is highly probable that I should have met with them had they occurred. Secondly, I have repeatedly been furnished with portions of the water, pumped up in haste, and at a time too when she was using it, and passing this siliceous matter, but I could never detect the smallest portion of silex in *mechanical suspension.*

But even granting that it might, and actually had been so introduced in this instance, such a solution involves still greater difficulties. If introduced into the stomach, does the silex undergo digestion, become dissolved, and thus enter the circulating mass, and so arrive at the kidneys? If so, it is evident that the kidneys must reorganise the silex to precipitate it in the crystallized form, for this can hardly be effected any where else*. It may be asked, could not the silex be carried whole, without undergoing digestion, to the kidneys, and ultimately make its exit with the urine? There are, however, a number of objections to such a theory. In the first place, fruit-stones, grape-seeds, &c., which resist the digestive powers, are always voided with the excrement, and have never been observed to pass with the urine. Secondly, before reaching the kidneys * they must pass with the chyle into the circulating mass, and hence go the round of the circulation. Now, we cannot conceive such hard gritty irritating matter pervading the more delicate tissues and finer vessels, without exciting violent inflammation and speedy disorganization.

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* But there is a still more weighty objection. If the seeds of pears, apples, and fruit-stones, &c., can resist the solvent powers of the gastric juice, we cannot well conceive them adequate to the solution of so refractory a substance as silex, at least under ordinary circumstances.

Upon reflection, and after close attention to the circumstances, I am disposed to attribute the appearance of this matter to a morbid condition of the kidneys themselves, either generating the substance itself by a morbid process, or else separating it from the drink or aliment, and aggregating it into the masses and forms under which it appears mixed with the urine.

In support of this view, I would observe that there is manifest disease in the kidneys as well as bladder, and a morbid condition of the uterus. There is evident tendency to urinary affections, attended, as is mostly the case under such circumstances, with an impaired state of the general health. There is a great analogy between the two cases, and between the general symptoms in each.

There is, however, one fact clearly established by this paper, namely, that siliceous sand may—however introduced, or whencesoever its origin—form, in some cases, a portion of gravel, and from which we may infer that it may sometimes form a portion of urinary calculi.

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* There can be no doubt that fluids sometimes reach the kidneys and bladder by a less circuitous route than the round of the circulation. The means by which this is effected, are at present involved in so much obscurity, that nothing decisive has been determined relative to this question. It has been supposed to be accomplished by transpiration, and if it has so, it is evident that silex in the solid form, though in a state of ever so minute mechanical division, could not be thus conveyed to the kidneys or bladder.

On the Word Bedolah, *or* Bedolach, בדלח. *By* Dr. J. J. Schmidt, *of St. Petersburgh.*

Every attentive reader of the Mosaic history of the creation must have felt some doubts respecting the true meaning of the word *Bdellium,* as it occurs in the second chapter of Genesis. Of the land of Havilah, which the river Pison "compasseth," it is said that "the gold of that land is good; there is bdellium and the onyx-stone." It is scarcely possible to believe that the bdellium here mentioned signifies nothing more precious than the gum now known by that name. In the eleventh chapter of Numbers it is said that "the manna was as coriander seed, and the colour thereof as the colour of bdellium." Here the gum might naturally be regarded as the object of comparison; yet many commentators, both ancient and modern, have supposed it to be the pearl, because manna is white, and Havilah is understood to have been situated on the Persian Gulf, which abounds in pearls. We certainly may, in this way, give an apparently suitable explanation not only of the valuable productions described as existing in the country through which one of the rivers of Paradise flowed; but more particularly of those objects with which manna, in regard to its colour, is compared. There yet remains, however, this very important objection to be got rid of. Why should pearls, if they be meant in the two passages cited, be there called Bedolach, and not דר, Dare, as in every other part of the Old Testament? It is evident that the interpretation "pearl" has only gained acceptance because that term, in the passages where Bedolach occurs, would produce an agreement in the sense; and all further philological confirmation is, therefore, supposed unnecessary.

In my opinion, the Bedola, or Bedolach, of the book of Genesis, is neither pearl, nor the gum called bdellium, and that it must denote some other precious thing,—perhaps, lapis lazuli. I am aware it may be asked, whether it can be admitted that Bedolach belongs to the mineral kingdom, since it is not expressly called a stone, as is the case, for example, with the stone shoham? But this objection is of little importance; for, were everything clearly and distinctly designated there would be at once an end of all obscurity and conjecture. How common it is, in speaking of precious stones, to call them by their own particular names!

Lapis lazuli is often made to perform an important part in the Indian cosmogony and mythology, and in those of Thibet and Mongul, which are borrowed from the Indian. Its Sanscrit, and also its Thibet name is *Veidurya*, or *Veiduryah*, which probably gave rise to its Mongul name—Bedurya. In this appellation of lapis lazuli the Mosaic Bedolach may be found without the least difficulty; for languages in general, and more particularly the Asiatic, afford so many examples of the conversion of R into L, and of V into B, or rather of the confounding of those letters, that no linguist can hesitate to admit the fact. Moreover, I am not ignorant that the sapphire of the ancients is pretty generally supposed to be lapis lazuli. This opinion, however, has no foundation in authority; but I am willing to leave to it all the credit to which it may be entitled.

Of the valuable productions of the mineral kingdom mentioned in the Buddah books, the following four may be regarded as the principal: namely, Gold, Silver, Veiduryah, and Padmaraga (ruby). The Ugyu, or Gyu (the oriental Zode), Marakata, or Marakta (Emerald), Crystal, and others, occur less often. According to those books, also, the eastern declivity of Sumeru, the fabulous mountain of the world and seat of the gods (by which some understand the high land of Thibet and Great Tartary), consists of silver; the southern, of veiduryah; the western of padmaraga, or ruby; and the northern declivity, of gold. In these books crystal is sometimes put for ruby, and, according to them, the principal eastern river, or the Ganga (Ganges), flows on silver sand; the southern, or the Sindhu (Indus), on veidurya sand; the western, or the Backtschu (perhaps, from *baktra* and *chu*, a Thibet word signifying "water"), on crystal sand; and the northern, or Sisita, on gold sand. It is said of these four streams that they spring at a short distance from each other,—from the sides, indeed, of the square Lake Map'am, in the centre of which grows the tree Jambu (rose-apple: it takes its name from that part of the world called Jambudirp), whose falling fruit serves as food for the serpent (*naga*), who inhabits the lake. We observe here, as in Genesis, four rivers take their source from a reservoir, only that in Genesis the direction of the course of each of them is not described.

I therefore conclude, that Bedola, or Bedolach, has, perhaps, a twofold meaning: that, in Genesis, chap. ii., it most probably signifies lapis lazuli; and that, in Numbers, chap. xi., it may mean the well-known gum with which manna is supposed to be compared.

On a Prismatic Structure in Sandstone induced by artificial Heat; and on certain Prismatic Rocks found in Nature, including the Columnar Sandstone of Dunbar. By J. Mac Culloch.

The connexion which may be inferred, between the artificial change in sandstone, that forms the subject of one part of this paper, and that natural arrangement of the same kind which occurs in certain rocks, has induced me to describe the whole in one memoir. I will not pretend to pronounce on the degree of probability that exists respecting this presumed connexion; but it is certainly such as to warrant a further repetition of similar experiments, and a further examination of analogous phenomena occurring in nature. However unfounded that supposed connexion may be proved, the results of the artificial process are intimately connected with so many analogous facts occurring in other solid substances, that it must be considered as only one example of a most important general law, the effects of which have scarcely yet been examined, and which opens a wide field of experiment to those whom leisure and opportunity may tempt to investigate it. If, by such experiments, that law may he ultimately shewn to have an extensive operation, such is its nature, that its action cannot be confined to the narrow extent of our furnaces and crucibles, That which regulates the affinities of the substances in these limited operations, has regulated all the chemistry of nature; and the globe itself is only ruled by those powers which determine the combinations of the least of its atoms.

Dismissing this part of the subject for the present, the artificial results which form the first part of this paper are interesting, even in an abstract chemical view, from their connexion with the analogous changes which so many other solid bodies undergo from the action of heat. In any point of view, a fact so solitary and unexpected is deserving of record.

With respect to the geological facts which form the second division of this paper, they are, indeed, too rare to be very satisfactory, since they seem rather to belong to the class of exceptions than rules. Yet I trust it will appear, that they are united by one common band which removes them from the class of insulated facts, and gives them a more general interest. Increased observations will probably add to the number; and by tracing various circumstances not to be found in these, by which they may be accompanied, geologists may ultimately establish a principle capable of explaining phenomena which have hitherto been a source of no small difficulty. Should even the phenomena here described prove incapable of being connected with any important general principle, their singularity and rarity render them worthy of record. The appearances at Dunbar, in particular, have been so much misunderstood, or so inadequately examined, that it seemed necessary to place them in a clearer point of view. I can only regret the want of materials sufficient to furnish a better connected train of evidence, on a subject which appears to comprise important consequences in geological science.

Some phenomena, which take place in glass, and in other solid bodies exposed to a continued heat incapable of bringing them into a fluid state, induced me long ago to undertake a series of experiments on this subject. Some valuable results were obtained, which I shall probably take some future opportunity of communicating through the medium of this Journal; but the impossibility of preserving a continued heat for a sufficient length of time in a common laboratory, ultimately caused these experiments to be abandoned. Among other substances, it was attempted to change the internal arrangement of those rocks, which have been supposed to owe their origin to igneous fusion, by the application of a heat incapable of producing that ultimate effect. But the defects of the furnaces already alluded to, rendered it necessary to abandon this train of investigation before any satisfactory or decided results were obtained. Thus the subject slept, without being forgotten, till it was revived by the circumstance which I shall now proceed to communicate.

In taking down a blast furnace at the Old Park Iron-works, near Shiffnall, which had been sixteen or eighteen years in constant work, the hearthstone was found to be broken, and, at the same time, to present a remarkable reticulated surface. On examining the fragments after removal, it was found that they were split into polygonal compartments, and some of them were therefore preserved. One of these was subjected to my examination by the Duke of Northumberland; in whose possession it now is, for the satisfaction of those who may wish to refer to the fact and its authorities.

The hearthstone, it is well known, lies at the bottom of the blast furnace, so as always to be covered with melted iron at a high heat, and to this heat it was unintermittingly exposed during the whole period of years above-mentioned. The kind of stone usually selected for this purpose is a fine-grained white sandstone, containing a very small proportion of argillaceous earth. It is either found, or imagined, to be the least liable to fuse or crack in this high degree of heat. I have not been able to procure natural specimens of the rock from which the hearth in question was made; and it is now impossible to ascertain its exact nature. It can only be concluded that, according to general practice, the hearth in question was made of this variety of sandstone.

The thickness of the fragment which I examined was about ten inches, which is understood to be that of the hearth. It is everywhere divided into prisms, which, in some places, reach through the whole thickness, but, in others, only penetrate to a certain depth; being gradually blended beneath with the solid mass. These prisms are sometimes tolerably regular; at others, their sides undulate, but so as still to fit each other, without intervals, wherever they approximate. Their diameter varies from one inch to the half of that, and they contain, generally, five, six, or seven sides. They are not absolutely in contact above, but they gradually approximate below; appearing to have been most separated where the heat has acted most immediately; though from this appearance it is not unlikely that this separation has been produced by mechanical causes. Their structure being tender, it is easy to imagine that the incessant motion of so heavy a fluid as iron along the surface of the hearth, had worn the sides of the prism so as gradually to permit it to insinuate itself among them, and thus to cause a separation towards the upper extremities. It is essential to attend to this circumstance, becauses it involves an important question respecting the columnar trap rocks. In the desiccation of clays containing much water, a division of the mass takes place, which causes it to separate into parts of an irregular prismatic configuration. This is the effect of drying; and, in consequence of the diminution of bulk thence resulting, the prisms are separated from each other by considerable intervals. In the columnar trap rocks, on the contrary, the prisms remain in perfect contact; proving that the configuration does not, in this case, depend on a general diminution of the bulk of the original mass. In the case of the sandstone in question, it is certain that it can undergo no loss of water sufficient to produce a separation of the mass from that cause; while the absolute contact of the prisms, where they have not been exposed to causes of waste, proves that the separation into these forms is the consequence of circumstances of some other nature.

On examining with a magnifying glass the texture of this sandstone in its present state, the most remarkable change is the want of angularity in the particles of the sand. It is true that this may have been the original character of the stone, but it is accompanied by an occasional vitreous splendour in some of them, which seems to prove that their surfaces have been slightly fused. It does not follow, however, that this is a fusion of quartz; it is more likely to be a glass produced by a combination of the argillaceous earth, where the two have been in contact. The small opaque particles which seem to be intermixed with the grains of quartz, may possibly consist of the supposed argillaceous earth of the original stone; but they are so difficult to determine, that I am even inclined to doubt if this specimen ever contained any clay, and whether it is not a simple sandstone. It has certainly undergone some change;

but that is of such a nature, that the lens, at least, is incapable of determining what it really is. It is necessary yet to observe, respecting the character of this burnt sandstone, that instead of being white, as in its original state, it is of a grey colour: and, on examining it with the lens, it is easy to see that particles of dark grey and transparent quartz are intermixed with the colourless and transparent; so as to produce this effect. The nature of this change in the quartz is not intelligible; and the specimen presents no other appearances that I could discover, capable of throwing any light on the subject.

It had formerly been proved, by the occurrences so common in glass-houses and iron-furnaces, among the glasses and slags which have remained long in the fire, that, in a long continued heat, or rather during a process of slow cooling, these substances were capable of crystallizing, so as, on solidifying, to lose their ordinary character of glass, and to assume the crystalline arrangement found in many kinds of rock. The experiments of Mr. Watt and others, on stones fused and treated in the same manner, confirmed their accidental results. But it remained to be proved that heat applied to rocks in a degree inferior to that which is required to effect their fusion, was capable of changing in an analogous manner the internal arrangement of their particles, as happens in the case of some other solid bodies. This was the object of the experiments which, as already mentioned, I undertook without success, but which this accidental experiment has proved to be possible.

But the change, in this case, is not a change of that which is properly to be considered the crystalline arrangement of particles. There appears to be no such arrangement in sandstones; nor are there any of the characters of true crystallization in the prismatic structure which has followed the action of the heat in this instance.

What the real nature of this analogy between the crystalline and concretionary structure in rocks may be, it is impossible to conjecture, ignorant as we are of the real nature of both these processes. In crystallization, we can see that the crystals, assuming the integral form or molecule, is the most simple, consists of similarly formed parts, divisible, as far as we have yet discovered, without end. In the concretionary structure, no such divisibility occurs; but the pseudo-crystalline form, which it assumes, is only divisible into shapeless parts, which bear no relation to each other, or to the general figure of which they are the constituents. Yet it is probable, that even the crystal must be constituted in the same manner; and that the ultimate particles, or atoms of matter of which it is composed, have no forms coincident with that of the molecule which they generate.

Whatever the differences between true crystallization and the concretionary structure in rocks may be, there is, at any rate, an analogy sufficient to show that both arrangements have been produced by certain attractions among small particles, tending to arrange them in particular forms. In the crystalline structure, that attraction has been exerted, either among simple atoms, or among definite combinations of these, free to act and move in the directions to which they are impelled by certain unknown laws, in consequence of the fluidity of the mass of which they formed the parts. In the concretionary arrangement, or the contrary, from every thing as yet known, the motions of the parts have been limited; either by their figure, bulk, or imperfect capability of motion; or in consequence of previous determined combinations, perhaps of a more complicated nature, and of the want of a fluidity, enabling them to assume the disposition to which they might possibly have tended under more favourable circumstances.

It is impossible, at present, to illustrate this obscure subject further; but it is evident, that on examining the various experiments that bear on this question, the action of fire is capable of producing both the crystalline and the concretionary arrangement.

In simple crystallizable bodies, heat changes the crystalline arrangement, even though the substance remains solid: in the case of this sandstone, it has generated the concretionary structure. In glass, it seems to produce a crystalline arrangement also; but, in this case, it is probable the change does not resemble that which takes place in metals, but that the nature of the combination of the integrant substances is altered. In fluid rocks, the parts being free to move, and the particles of the different substances to enter into new combinations, it generates the crystalline structure; and here, also, we may observe that there is an analogy between the effects, the same cause producing the crystalline structure where the rock is fluid, and the concretionary where it does not lose its solidity.

If, therefore, the limited experiments which have been hitherto made in our laboratories have thrown light on the crystalline structure of certain rocks, by proving that this may have taken place from a state of igneous fluidity, the accidental experiment here described may, perhaps, tend to throw a similar light on the concretionary structure of rocks. The experiment, indeed, is as yet solitary; but even a solitary experiment is the result of a law, and that law must act in every other case, where the circumstances are the same. We may therefore infer, without much hazard of error, that the long-continued application of a certain degree of heat to a sandstone of certain composition, will, in all cases, under identical circumstances, generate in it the prismatic concretionary structure. It will hereafter be shown, in the second part of this paper, how this principle may be applied to the solution of some difficult cases in geology.

How far we may venture to extend this principle from sandstone to other rocks, is a question to which no decided answer can be given. It may easily be conjectured that I here allude to the trap rocks, which present examples of the concretionary structure the most remarkable and frequent among those with which we are acquainted.

To illustrate this subject as it merits, it would be necessary to put it to the test of direct experiments modelled on that which is here recorded. What time would be necessary to produce a result, if a result is to be produced, is uncertain. I have attempted in vain, by inquiries among iron-founders, to learn what length of time the exposure to heat was required to produce the prismatic configuration of the hearth sandstone. But the destruction of a furnace is a rare occurrence, and the time which it remains at work is such, that there seems no possibility of discovering the *minimum* of time required for this effect. Still, the experiment might be tried in the hands of those who are conveniently situated for that purpose, by exposing various trap rocks, as, for example, basalts, of sufficient bulk, to a heat short of that required to fuse them, and for a sufficient length of time, to be determined only by repeated examinations. Should such a result take place as that which has occurred in the sandstones, we should then be in possession of a valuable fact from which to deduce a train of geological reasoning respecting the figured traps, which must now be supplied by conjectures and analogies. What may be offered on that subject I shall now defer, till the natural appearances, which resemble the artificial one already described, have been detailed.

I shall now, therefore, proceed to describe some appearances in nature, which appear not only to illustrate the preceding experiment, if such it may be called, but to show the connexion which it has with similar phenomena in the structure of rocks. That connexion it must, however, be admitted, hangs on a hypothetical view of a cause; or rather, on an inference regarding the probable action, in this particular instance, of a certain state of things which is now almost universally admitted to have existed. But the nature of the reasoning on this subject will be better understood when the appearances themselves have been described.

That substance known by the name of "columnar ironstone" is probably familiar to most mineralogists. It is found under many different modifications, and these principally relate to the sizes of the prisms of which it is composed. For the objects of the question in view, although it is not useless to inquire respecting the causes of this peculiar configuration, it is impossible to reason very satisfactorily from it, unless the geological situation of all such specimens were carefully examined. Unfortunately, the only instances with which I am practically acquainted are those which occur in Arran, which I have described in my work on the Western Islands of Scotland.

These examples are noted for the large size of the prisms, and for the facility with which they separate into short joints; while, in one of them, there is further a peculiar character produced by a groove or channel on the ends of the prisms, and parallel to their sides. These peculiarities, however, seem to have no particular connexion with the question now under consideration.

It is proper to remark, however, that this bed of ironstone, as it is called, might equally be termed a shale, in which the red oxide of iron abounds in an unusual degree. Such a composition of clay and iron is, indeed, that of all iron stones. In this place many of the shales are highly ferriferous, being of red, yellow, and purple colours; so that while the exact limits of ferriferous shale and argillaceous ironstone cannot be defined in the abstract, neither, in this place, can it be said that many of these beds are not formed of the latter substance. The shales also pass so gradually, in respect to composition, into the columnar ironstone which occurs near them, that the differences are often not perceptible. The columnar bed under review might, indeed, with equal propriety, be called a ferriferous shale; or else the shales of the same character should rank, like it, with the ironstones.

I must remark that the surface of the columnar ironstone in this place being exposed, there is ready access, not only to this bed itself, but to all the surrounding rocks. These rocks are sandstones, shales, and limestones, of various characters; all of them appertaining to that series of red sandstone which forms this quarter of Arran, and which extends across the whole of Scotland to the eastern shore. Besides these, veins of trap occur in the immediate neighbourhood; some of them intersecting the strata, and others following the same parallel course, so as to appear interstratified with them. It ought still to be observed, that although masses of superincumbent trap are not found at this particular place, they abound in other places in Arran, not far off; forming, indeed, a considerable portion of this island. As the reasoning which it is intended to apply to this phenomenon applies equally to the case next to be described, I shall not now enter on it, but defer the whole argument till the history of this also has been given.

In the island of Rum there is a mass of sandstone, similar to that of Sky, which, in some places, is covered by trap rocks of various characters. Under the hill of Scuirmore, this sandstone lies under that basaltic rock so remarkable for the quantity of heliotrope which it contains. In one place the bed i divided naturally into regular prisms, of a diameter not exceeding two or three inches; which, being detached, may often be found on the beach. It is not possible to ascertain the extent of this appearance, because the greater part of the cliff is absolutely inaccessible. Standing alone, it is not a circumstance, perhaps, to have excited much interest; but when connected with the phenomena next to be described, it forms a valuable addition to a collection of facts as yet, it is true, rather too limited to be of a very satisfactory nature.

It has long been known, that a collection of columns of considerable size exists at the pier of Dunbar; and it has been examined, perhaps, by every mineralogist who has resided in or visited this country. This rock has been known by different names, having been called a red basalt, and a columnar jasper. It will presently be seen that it bears no relation to basalt, being a stratified rock; and that, instead of consisting of one substance only, the prisms are formed of different minerals or rocks, appointed by the common bond of juxtaposition and form. The probable causes of their variety, and of their real nature and origin, will be rendered sufficiently apparent when their true geological connexions are described.

The whole of this shore consists of that red sandstone already mentioned as reaching from Arran across the centre of Scotland. It is true that, between the two, there is no continuity of connexion visible; but their relations to the subjacent and superincumbent strata, and their characters, are, in all essential points, the same. This sandstone is the old red sandstone of certain geologists.

The substances of which the series consists at this place are, principally, a red argillaceous sandstone, with which are interstratified red ferriferous shales, common lead-blue shales, white sandstones, and some slender laminæ of an impure calcareous rock. The principal limestones lie higher up in the series, which, as is well known, is followed by the coal-field of this coast.

Another circumstance respecting the general structure of this country must yet be noticed, to render the geological description complete. It contains, in several places, insulated and superincumbent masses of trap, together with veins of the same rock. The Bass, Traprain Law, and North Berwick Law, are the most conspicuous of these: but many of smaller dimensions, scarcely raised above the general surface, occur in other places. It is further obvious, that many of these detached masses of trap are undergoing a rapid decomposition; gradually mouldering into soil, and leaving exposed, or at least only covered with loose clay and earth, the stratified rocks which they once concealed.

I have yet to remark, that the general inclinations of the sandstone strata are various, but almost always inconsiderable; and this comprises all that appears necessary to render the geological structure of this district intelligible.

The strata of the red sandstone, as they appear near Dunbar, dip to the south-east, or thereabouts, at an angle of nine or ten degrees; and, in most places along this shore, they present a very consistent regularity. Immediately at the town, however, they are confused and irregular for a small space; and here also there are intermixed with the red strata some beds of white calcareous sandstones veined with red.

In carefully tracing these beds to the pier, where the columnar appearances already mentioned are found, it is easy to see that this mass of prisms is a portion of one of these beds, or that the bed is, in this particular place, divided by a prismatic configuration. The area of the columnar part is such as might be contained in a space of 80 or 100 yards by 30; but accuracy in this point is of no consequence: neither is it possible to be obtained, as there is such a gradation from the simple undivided stratum into the prismatic part, that the limits of the latter cannot be fixed.

It is, indeed, easy to see, on a careful inspection, that the prismatic configuration is assumed by the common sandstone bed in a very gradual manner. The mere indications of an irregular vertical division are first seen; and these are followed by a definite outline on the surface, making the figure of a prism which is still incapable of being separated from the surrounding parts. By degrees, these markings increase, so as to leave hollowed lines in the rocks; and, in a further progress to the perfect structure, the prisms are not only seen to be distinct, but, having been irregularly broken off at unequal altitudes, they form an uneven columnar floor resembling the well-known causaway of Staffa. This very perfect part of the structure does not, however, occupy a space of more than a few square yards.

As the columnar division takes place in the manner most common in the trap rocks, namely, at right angles to the stratum, the vertical inclination of the protruding columns is ten degrees, or thereabout. Their diameters vary from eighteen inches to two feet, or more; and, as far as can be discovered, their greatest altitude is about ten or twelve feet. It is not certain, however, that this is their total height, or the greatest thickness of the prismatic part of the sandstone bed; as a great part of the surface has been broken or worn away during the lapse of time. The angles of the prisms vary in number; but, like the cases most frequent in the trap rocks, forms of five, six, and seven sides are the most numerous. They do not

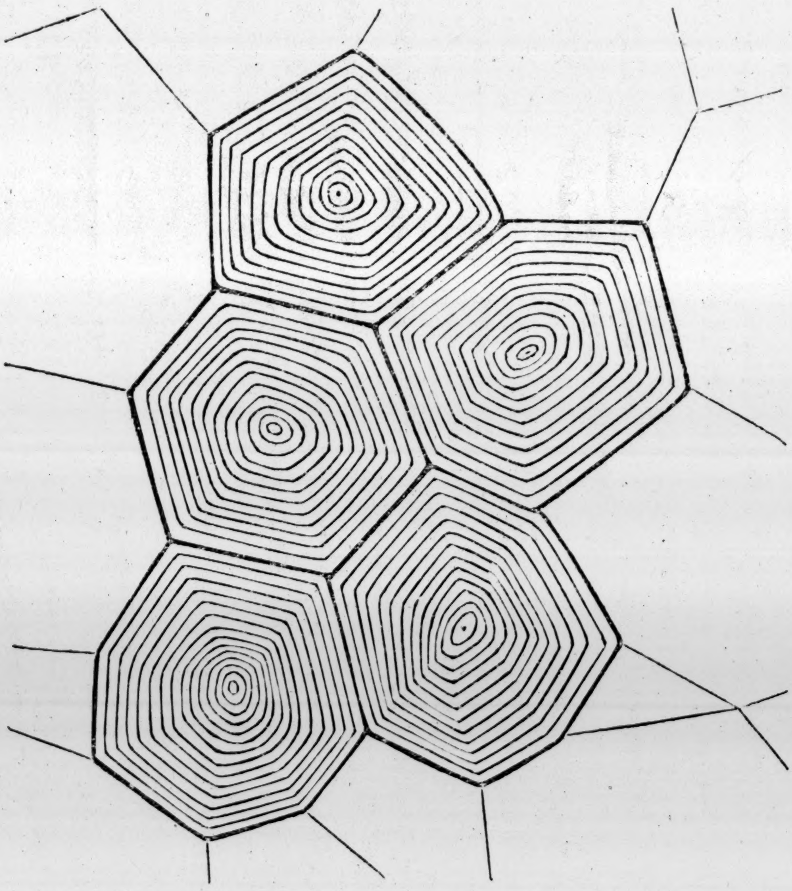

Fig. 1. *Concentric Structure of the Columns at Dunbar* appear to be anywhere jointed, but they break at right angles to their axes. Neither do they present any marks of that peculiar channelling on the surface which occurs in the shale of Arran.

Independently of this decided configuration, there are some indications of an internal structure in this sandstone bed, connected with the prismatic form, which it is necessary to notice.

Where the first indication of the future division into columns takes place (see *fig*. 1.), the primary or bounding lines by which they are indicated are often filled with a series of concentric lines, distinguished by some differences both of colour and texture, and commonly by white lines on a red ground. This circumstance presents an interesting analogy to the case of the basalts, in which, from their eventual decomposition in successive crusts, it appears probable that they possess an

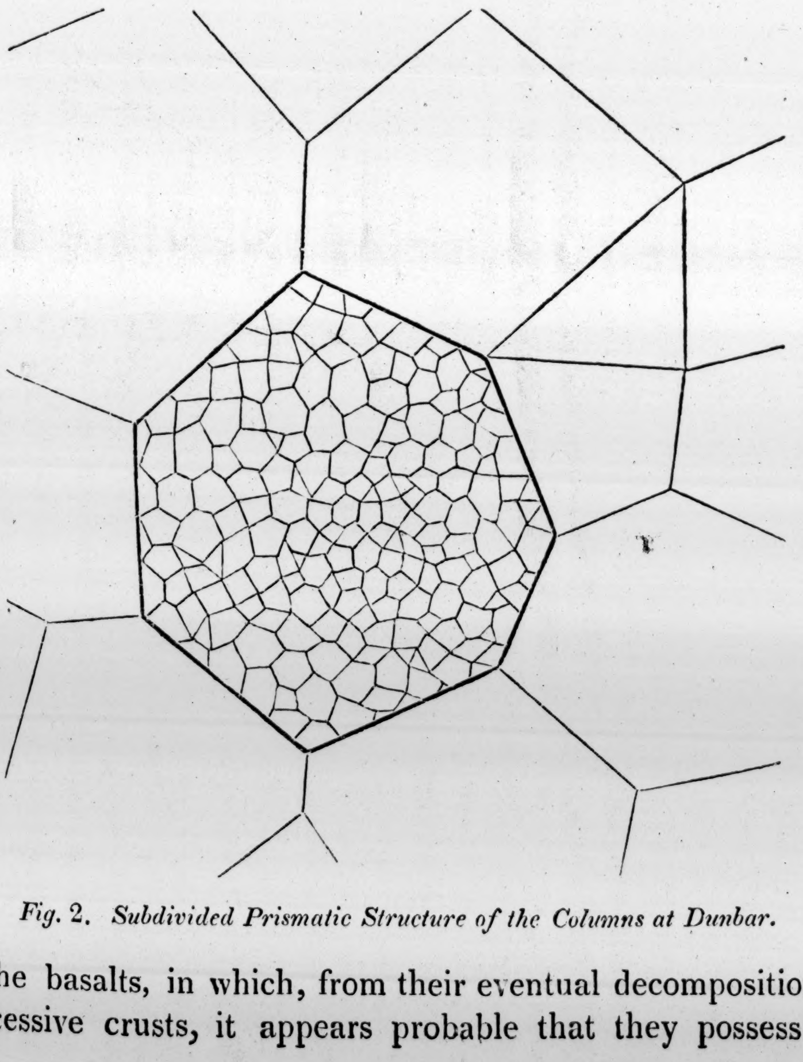

Fig. 2. Subdivided Prismatic Structure of the Columns at Dunbar.

original internal corresponding structure, although that is not visible in the fresh rock, perhaps in consequence of the nature and uniformity of the colour. Having, in another communication, stated the facts and arguments on both sides respecting this process in basalts, I need not notice it further here. But it is plain in the case of this sandstone, that any exfoliation which the prisms may eventually undergo will not be the consequence of superficial decomposition from the action of the air, but will have its foundation in a peculiar concretionary structure of a laminar nature concentric to the axes or circumferences of the prisms.

Another remarkable variety of internal concretionary structure also occurs here (see *fig.* 2). In this a minute columnar tendency within the principal column is indicated in the same manner by variations of colour. Whether this is ultimately to become actually divided into small prisms cannot be conjectured; but no marks of such division are as yet to be seen. But I must remark that this case also presents an exact analogy to one occurring in a trap rock (hard claystone) in Morven, where a similar large division of the rock is subdivided into prisms of half an inch, less and more, in diameter, which are capable of being removed to the depth of two or three inches. A parallel case occurs also at Strontian. It is, lastly, necessary to describe the variations of mineral character which this bed of sandstone undergoes, when it will be seen what the true nature of these disputed columns is.

The bed, in its ordinary state, is, as already remarked, the common red sandstone of the country, and consists, either of red quartz sand alone, or of quartz mixed with red clay, or with calcareous earth, or both. It is often so charged with clay, as almost imperceptibly to pass into a red shale. In the transition, there are, of course, many arenaceous, scarcely definable substances, of an intermediate nature. Although the general mass of the bed is red, there are white portions intermixed; but these are small in quantity when compared to the red part of the rock.

It is very perceptible that the greater part of this bed, where it is here accessible, is harder than this rock generally is found to be in this neighbourhood. But it becomes decidedly more indurated where it approximates to the columnar parts. These are, in many places, extremely hard; but that character is not universal, and the causes of these differences are very easily explained, when the variations of character in the unaltered part of the bed are considered. Some of the columns thus consist of sandstone highly indurated, while others are formed of the same indurated shale as that of Arran already described, and might thus with the same propriety be called columnar ironstone. In some places the columns consist of jasper; that is to say, the argillaceous sandstone is here indurated to such a degree, as to put on the appearance and characters of that rock. It is thus plain, that the term columnar jasper has been improperly applied to this whole mass of prisms, as the occurrence of this particular substance is but partial. It is indeed so irregular as not even to exist necessarily throughout the whole of any one column, or a single column is not always formed of one substance; as for example, of jasper, of ferruginous hard shale, or ironstone, or of indurated sandstone. On the contrary, the jasper often forms a very small part of one column, and all these substances are sometimes found intermixed in the same. This is a circumstance, therefore, which may easily be understood from considering, as already hinted, the mixed and irregular nature of the sandstone bed where it retains its natural form and disposition.

It is now necessary to inquire respecting the probable cause to which these prismatic concretions owe their forms and origin. It is no assumption, if a fact, to use this language. If there is anything certain in the whole range of geological science, it is that the secondary strata have been deposited from water, at least essentially, in their present predominant forms; and from this cause they assume that simple stratified appearance which is the natural result of this process, and on which it is quite unnecessary to dwell. But, in many places, they are partially modified, or present appearances of a limited nature, inconsistent with their prevailing characters, and probably, therefore, the consequences of actions of a different and local nature, simultaneous with, or posterior to their deposition.

The structure in question must be ranked with these, as no depositions of earths from water, under the usual circumstances, can be conceived capable of producing the effects under examination. Their partial and limited nature also proves that two causes have acted in this case.

The reader who has attended to the statement at the commencement of this paper, is doubtless prepared to expect that the prismatic structure in the instances which have now been described are attributed to the action of heat. I shall attempt to arrange the arguments which I have to offer in support of this opinion in the most intelligible form. I make no scruple in assuming that which all rational geologists now admit, namely, that the trap-rocks are the produce of igneous fusion; and, in taking this for granted, the source of heat, so far as it concerns these sandstones, is established.

Now, in very many cases where trap rocks are found in contact with certain strata, these are found to have undergone particular changes within certain limited distances of the places of contact. Thus, shale is converted into siliceous schist, earthy limestone is crystallized, highly argillaceous limestone becomes chert, and various argillaceous rocks pass into varieties of jasper.

The relations between the several varieties of this latter substance thus produced, and the various natural or original strata from which they have been formed, are those which particularly bear on the present question. The purer clays are found to be converted into fine jaspers of a resinous aspect, as I have shewn in a former paper in this Journal. The argillaceous white sandstones are changed into jaspers of a less perfect character, which vary also in the same place, according to previous varieties in the composition of the different beds from which they have been derived, and to which they may still be traced. The red argillaceous sandstones are converted into red jaspers, of characters equally varying, according to the previous nature of the original rock, or to its distance from the source of the heat; and the examples of this are so familiar to all practical geologists, as not to require being specified. The shales which contain much siliceous matter, are, in the same situations, converted into ironstone and into jasper; and, in the same manner, the incompact highly ferruginous clays become, according to their capacity and other circumstances, one or other of these substances. That the effects there enumerated take place in innumerable instances is fully demonstrated. That they sometimes do not occur where they might be expected is equally admitted. But it is not necessary to show reasons for these exceptions, as the discussion would be here out of place, and has often been before the public in many different forms.

It is therefore proved by natural appearances, or in the great experiments of nature, that the action of heat existing in the trap rocks is capable, among other changes, of converting red argillaceous sandstone, and ferrugino-siliceous shales into jasper, and the latter, under other modifications, into ironstone. It was also shewn in the first part of this paper, that the action of artificial heat was capable of producing the prismatic structure in sandstone.

It remains to produce an instance in nature where the presence of trap and the prismatic form in the sandstone near it concur; and this example is found in the instance in Rum above mentioned. It will probably be found in many other places, when the facts and reasonings which this paper now promulgates for the first time shall become known to geologists.

As the action of heat, therefore, or the presence of trap, and its presumed influence over the strata, have been shewn capable of producing, separately, either the prismatic configuration, or the change to jasper in certain modifications of sandstone, and that instances of each of these have been adduced, it does not seem at all illogical to conclude, that the same cause may produce both these effects united. I must regret that I cannot produce an actual instance of the contact of trap with this union of the jaspideous character and the prismatic structure, but future researches will probably discover them.

In defect of this, we can only be guided by reasonings from analogy, in attempting to explain the causes of the peculiar nature of the three examples described in the second part of this paper.

The case of Rum is indeed proved; since, as already observed, the trap is found in contact with the prismatic sandstone. In Arran, the presence of trap, in various parts of the tract in which the prismatic ironstone occurs, is evident; and from the facility with which this rock is often decomposed and removed, and the great destruction of substances here visible, it is not improbable that some mass of it has once been in contact with the columnar part of the bed of shale which forms the ironstone under review.

In describing the country about Dunbar, I shewed that detached superincumbent masses of trap occurred in various places, and that, in many, they gave striking marks of rapid decomposition. The ultimate removal of such masses is proved by their gradual evanescence in other parts of Scotland; in many districts of which they may be traced gradually diminishing in size as they recede from some larger mass, till they vanish, leaving proofs of their former existence in this peculiar nature of a soil, which those who have once been intimate with it will never fail to recognize. The conclusion to be drawn from this reasoning is, that one of these masses of trap has originally covered the prismatic sandstone of Dunbar, and that to the influence of this is owing the change both of structure and mineral character which it has undergone.

In terminating these remarks, I do not feel it necessary to account for exceptions; for it may be said, that this effect, if depending on that cause, ought to occur more frequently. The common answer to this often-repeated objection against all general conclusions from particular facts is sufficient. Nothing can be pronounced an exception till all the concomitant circumstances, or those capable of modifying the presumed results, are known.

Of these we know little at any time; and the objection, such as it is, applies equally to every case where the influence of trap on the neighbouring rocks is visible.

It now remains to inquire how far the preceding facts and reasonings can be applied to the often-discussed question of the prismatic configuration of the trap rocks. Here we have, at least, some strong analogies to guide us. It is certain that, after such fluid masses had become solid, they must, for a long period, have retained a considerable degree of heat, and undergone a process of slow cooling. It has, indeed, been supposed by many, that the prismatic configuration in the traps has taken place during the act of consolidation, or that it was a crystalline process. To apply that term to it is incorrect; it is a concretionary structure, and, from the examples in sandstone which have been described, it is more likely to have taken place in the solid than in the fluid mass. The same reasoning applies to the prismatic lavas, which, indeed, are themselves sufficient to prove that this structure is the result of heat, at whatsoever period and state of the lava it may have acted.

Finally, as in the Dunbar sandstone the concentric concretionary structure of the prisms has been shown to exist, and that this arrangement is common in the columnar traps, as is proved by their decomposition, it is probable that in the latter, as in the former case, it has been produced during the solid state of the rock.

It is impossible to illustrate this subject further, in the present state of our knowledge; nor is it very likely that any satisfactory proofs of it can be derived from nature. It is not easy, at least, to conjecture how such proofs can be discovered. That it might be put to the test of direct experiment I have already suggested; and repeating the hope that such experiments will be instituted, I shall now conclude this communication.

Experiments on Indigo.

[From a Calcutta Journal.]

Chemists in Europe, who have engaged in the examination of indigo, have generally had to deal with the prepared colouring matter as manufactured for the market; and have therefore limited themselves to the separation and measurement of the foreign ingredients with which it was contaminated,—to the properties of the pure colouring matter itself,—and to the analysis of its chemical composition. The rationale of what passes in the process of the manufacture may be, and has been, deduced with tolerable certainty from the discoveries thus made; but where we have the whole fermentation carried on among us on an immense scale,—when we have the indigo in its nascent state, and in its colourless soluble state too, capable of being submitted to tests and processes, it becomes a matter of curious interest to follow the changes of this singular substance, and compare them with the theories formed in the laboratories at home.

It is, however, more than a mere matter of curiosity to set on a sound basis the causes of the different manipulations, and to examine the real effect of processes in which different manufacturers have a diversity of practice;—although it must be confessed that, setting aside the difference of quality in the plant, from season, care in its cultivation, soil, and other causes, the business of the vats is so simple as to allow of little deviation in practice or result.

The most convenient manner of bringing before the reader the various subjects of a short series of experiments which I made this season, in conjunction with an intelligent friend engaged in an indigo establishment, will be to incorporate them with a relation of the general process of manufacture; but I think it will save a good deal of repetition and explanation, to give in the first place an epitome of what has already been written on the subject; that is, on the qualities and properties of indigo itself, which have been elaborately examined by Bergman, Berthollet, Chevreul, Thompson, Crum, and latterly by Berzelius. Their notices extend to minute details of the action of every neutral salt, acid, and alkali of the chemical calendar; but the general results which are likely to be useful to the manufacturer may be condensed into a very small space.

Indigo is a definite vegetable product, which appears to exist, in greater or less quantity, in a variety of plants, or rather the elements of which it is composed are found in these plants; for it is not until the juices of the vegetable begin to act upon one another in fermentation, that indigo is developed. It might at first be supposed that the green colour of plants was connected with the presence of indigo; but this is not the case, as after the leaves have been steeped in the vats, they retain entirely their original colour.

When first dissolved from the plant, the indigo is in a colourless state, and is readily soluble in water; but it becomes blue on absorbing oxygen from the air, and appears then to have assumed the nature of a peroxide, for it is very unalterable, quite insoluble in water, alcohol, ether, saline infusions, alkalies, and dilute acids. Concentrated sulphuric acid alone acts as a solvent, without changing its nature. Nitric acid converts it into a yellow bitter principle. Acetic acid by degrees deoxydizes it. Chlorine also destroys its colour immediately.

In saying that concentrated sulphuric acid effects no change upon indigo, I am not strictly correct. It does not destroy the colouring matter, or cause a decomposition, as would be the case with most other vegetable compounds; but the researches of Crum and Berzelius have rendered it probable, that three different modifications of indigo may be brought about by the agency of this acid, differing from one another in the quality of oxygen or of water in their composition.

The three modifications are thus distinguished :—

1. The *Pure Indigo*, which is obtained by sublimation from the crude cakes at a temperature of 550°. This is crystallized in long flat prismatic needles; has a copper colour by reflected, and a fine blue by transmitted light. It sublimes entirely at the temperature stated, without residue: its specific gravity is 1.35.

2. *Cerulin*, or *Saxon Blue*, is obtained by digesting the former substance in sulphuric acid: it is supposed by Crum to have lost its water of composition thereby. It is soluble in water, but is precipitated by most of the neutral salts.

3. *Phenicin*, or *Purple Indigo*, is obtained by suddenly diluting the sulphuric acid which has begun to dissolve indigo. It separates as an insoluble powder, which when filtered and washed, is soluble in pure boiling water, and may be procured as a precipitate again by the addition of any neutral salt. Phenicin is supposed by Berzelius to be an intermediate state between soluble and insoluble indigo; but Crum asserts that 100 parts of indigo will yield 120 of phenicin.

The indigo of commerce most probably ranks under one of the two latter denominations, or, perhaps, both of them; for there can be little doubt that some new combination of elements takes place in the sublimation of the "pure" or "crystallized indigo," since with the utmost care, not more than one-fifth of the weight of crystals can be procured; and, during their formation, first, aqueous vapour, then gas is extricated; and a red coloured oil; and a large residue of charcoal is left behind; whereas the crystals, once formed, are volatilizable without any loss or carbonaceous residue. As the crystallized state, and the resistance to destructive agencies at an elevated temperature, are the sure signs of a definite, and generally of a simple atomic composition, we should expect to find tolerable accordance in the analysis of the crystals of indigo. I have only been able to find two made by different chemists, but they are greatly at variance with one another*.

Pure indigo, analyzed by Thomson			*by Crum.*
Consists of	Oxygen	46.154	12.60
	Carbon	40.384	73.22
	Azote	13.462	11.26
	Hydrogen	——	2.92
		100.	100.

The other varieties are stated by Crum to be thus composed:

	Cerulin.	*Phenicin.*
Oxygen	29.0	21.6
Carbon	57.8	64.9
Azote	8.4	9.5
Hydrogen	4.8	4.0
	100.	100.

If the composition given by Thomson be correct, indigo ought to be a remarkably easy substance to analyze; since it would merely be necessary to submit it to a destructive heat,—to determine the proportion of carbonic acid, carbonic oxide, and azote,—and to weigh the surplus carbon. The quantity of oxygen in the analysis of Mr. Crum will be proved hereafter, I think, to be too small.

From the almost total insolubility of pure indigo, it becomes a very simple matter to separate the foreign ingredients which are found with it, and consequently to obtain it in a state of purity. All that is necessary is to boil it for a sufficient time,

1st, In pure water, which removes yellow extract, green matter, &c.

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* Dr. Ure gives in the Appendix to the second edition of his Dictionary, Ox. 14.3, Carb. 71.4, Az. 10, Hydr. 4.4. But he thinks these numbers may require a little alteration.—Ed.

2nd, In alcohol, which carries off red colouring matter, and resin.

3rd, In dilute muriatic acid, which takes up lime, oxyde of iron, and magnesia, &c.

These methods will not, however, remove sand and clay, the presence of which can be detected by burning a portion of the blue cake.

The quality of indigo seems materially to have improved of late years, for Chevreul only obtained 45 per cent. of true colouring matter from Guatimala indigo; Bergman found 47; Brande states it at 50 per cent.; whereas in two specimens which I analyzed in Calcutta, one contained 75, the other nearly 80 per cent. of pure blue: neither were these the finest produce of the market.

Deoxidizing substances, such as the sulphurets, protoxide of iron, phosphorus, the sulphites, &c., have the power of depriving indigo of a part of its oxygen, whence it becomes again soluble in water or alkaline leys, preserving the power of regaining its colour the moment it afterwards meets with oxygen. It is thus that the dyers are enabled to prepare a solution for the purposes of their art. They are said to make use generally of sulphate of iron and slaked lime, which are mixed up intimately with the indigo, in the proportions of two of the sulphate, 50 of lime water, and 1½ parts of indigo, and then boiling the mixture in water. The colour of the solution is yellow.

Indigo thus deprived of its oxygen has been called Indigogene by Liebig: Berzelius calls it "Reduced Indigo." It is said to be obtained with facility in precipitation from the dyer's solution by muriatic or acetic acid, to which a small addition of sulphite of ammonia must be made, to prevent the access of oxygen.

Liebig asserts that this indigogene, at a moderate temperature, absorbs oxygen suddenly from the air with a species of combustion, and that in the mercurial eudiometer the absorption is found to amount to 11½ per cent. of its weight. The colour changes simultaneously from white to a rich purple.

Indigogene is soluble in the caustic alkalies and lime water, in which it may be kept without alteration for any period, provided air be entirely excluded. The solution in potash forms an excellent eudiometrical liquid, as it absorbs the oxygen of the air with great avidity, without giving out any gas to complicate the result. I had occasion to remark this circumstance in the course of the experiments hereafter described; and as the solution may be prepared direct from the manufacturer's vats in any quantity, it may prove a valuable discovery in the laboratory as a useful substitute for Sir Humphry Davy's eudiometrical liquid, composed of green sulphate of iron, saturated with nitrous gas, which is difficult to preserve, and may give out a little nitrogen in its operation. It remains, however, to ascertain how long the alkaline solution of indigogene will keep unaltered.

Having thus briefly enumerated some of the principal properties of indigo, as a substance *sui generis*, (and there appears to be no other vegetable product which resembles it in containing so much oxygen without being acid, and in the absence of hydrogen, and the presence of azote,) I shall proceed to the experiments on the process of manufacture, which form the immediate subject of this paper.

MANUFACTURE.

"The plant, after being cut and carried to the factory, is thrown into the steeper or superior vat, where it is pressed with timbers adapted to the walls of the vat, to prevent its rising in the water, which is then filled in from a reservoir, so as completely to cover the plant."

During the fermentation which follows, bubbles of gas rise to the surface, to ascertain the nature of which our first attention was directed.

The bubbles collected from the vats were found to contain merely 7 or 8 per cent. of carbonic acid; the remainder being common air, with from 12 to 18, instead of 21, per cent. of oxygen. Earthen vessels were inverted, and left with their mouth immersed all night in the vats; but the air in them was found unchanged. When bottles were partially filled with the liquor of the vat, and well closed, the air, after a day, was always found in them contaminated with about 18 per cent. of carbonic acid,—the rest being common air, without diminution of oxygen, excepting that portion due to the original air now replaced by the carbonic acid gas.

By way of examining in a more unexceptionable manner, the gas given out during fermentation, the operation was conducted on a small scale, by steeping some of the leaves in a glass cylindric vessel furnished with stopcocks and tubes to convey the gas, which should be emitted, into a glass receiver.

After twenty-four hours, (for the process of fermentation does not proceed so rapidly as in a large vat,) the quantity of air given out by twelve sicca weight (= 2160 grs.) of leaves, was 26.1 cubic inches: the disengagement still went on, but very slowly.

The gas was analyzed at two different periods: towards the middle of the disengagement it was found to be composed of

Carbonic Acid . . .	27.5
Oxygen	5.8
Azote	66.7
	100

And at the conclusion it contained—

Carbonic Acid . .	40.5
Oxygen	4.5
Azote	55.0
	100

Probably the atmospherical air of the apparatus, as well as that contained in the water, and that which remained entangled among the leaves, may account in some measure for the presence of the oxygen and azote in the first analysis; but the large proportion of azote, especially in the second experiment, so much surpasses what might be expected from this source alone, that it seems necessary to attribute it, in part, at least, to an emission from the leaves during the fermentation; or probably the leaves, still retaining atmospheric air on their absorbent surfaces, convert the oxygen thereof into carbonic acid, and allow the azote to escape. After the disengagement has ceased, it will be seen by the next experiment that the proportion of azote decreases very much. Some of the fermented liquid of a large vat was well boiled, and the gas collected over water: on analysis, it proved to be composed of—

Carbonic Acid . . .	78.
Oxygen	2.3
Azote	19.7
	100

And in a second experiment, conducted with greater care to exclude external air, the result was 86 per cent. of carbonic acid, and the residue contained too feeble a portion of oxygen to explode with the electric spark on the addition of a requisite proportion of hydrogen.

In no case, therefore, does the extricated gas in practice appear to be pure carbonic acid; but from the prevalence of the latter increasing with the precautions taken to exclude common air, it may in fact be the only gas strictly due to the fermentation properly so called, the remainder of common air having been suspended in contact with the leaves, and being deprived of a portion of its oxygen by the free carbon, or by the liquid in its passage to the surface. No carburetted hydrogen or other combustible gas was found among the gaseous products.

The indigo manufacturer does not wait until the extrication of gas is concluded, but withdraws the liquid from the steeping-vat as soon as he considers it to be sufficiently fermented; judging either from the smell, from the greenish tint of the liquor on the surface, or from the formation of an iridescent scum on the bubbles of gas. In fact, when the liquid, which is of itself of a bright yellow colour, begins to assume a greenish tint, it is evidently caused by an incipient precipitation of the blue colouring matter; and it would be attended with a loss of produce, to allow this precipitation to take place in the steeping-vat among the leaves and branches of the plant.

"The length of the fermentation depends upon the temperature, the weather, the wind, the water employed, and the ripeness of the plant; it may last in common cases from seven to fifteen hours. It is generally longer when the temperature is high, the weather cloudy, but not rainy; the wind eastward and moderate, the plant ripe and fresh."

Upon these several points, constant experience leaves little for the experimentalist to advance; but it may be remarked, that almost all the criteria of a good fermentation, as far as the weather is concerned, concur in one simple principle,—the prevention of the water of the vat from being cooled; for the *west wind*, being dry, cools it by evaporation; strong wind does the same; clear sky cools it by radiation; and rain, by the low temperature of rain-water. Thermometers placed in the vats did not present any satisfactory results; the mean temperature was about 85 degrees Fahrenheit.

"The fermented liquid is drained off into inferior vats, which are called the 'beating vats.'"

At the time when the vat is opened, the liquid is found by experiment to have a specific gravity (at the surface) of 1001.5, and at the bottom of the vat, 1003.1. The leaves appear to have lost nothing, being as green and fresh as when they were first strewed in the vat. By carefully weighing a portion of leaves, however, before immersion, and washing and drying them in the air after it, they were found to have lost more than three quarters of their weight: of this loss the greater part was water, which they apparently cease to have the power of retaining when the soluble juices have been withdrawn. The solid matter taken up by the vat amounts to between 12 and 14 per cent. of the weight of the leaves.

When the fermented liquid runs off into the lower vat, a frothy extrication of gas covers the whole of its surface. It is a good sign if the froth, in subsiding, assumes a rosy tint, which is in fact nothing more than a very thin film of indigo, and it proves that the deposition is ready to take place.

"In this vat the liquid undergoes a beating for about two hours—it is continually stirred about and agitated by a number of men, either with their arms or with a sort of short oar."

The object of this operation appears to be threefold. In the first place, the agitation extricates a large proportion of the carbonic acid gas which still remains combined with the liquid:—in the second, it exposes fresh surfaces continually to the contact of the air, whence the oxygen is rapidly seized by the nascent indigogene:—and thirdly, it coagulates the fecula of the indigo in larger grains, so as to render it more easily precipitable.

By way of understanding more clearly what takes place in the beating vat, a number of bottles were at different times carefully filled with the yellow liquor, just as it was ready to be drawn off from the upper vat, for experiments in the laboratory.

Neither keeping, boiling, the addition of acids or alkalies, nor even putrefaction, appeared materially to affect the power of depositing Indigo,—the solution always became blue the moment it came in contact with oxygen.

It may be remarked, however, generally, that the longer the liquid had been kept, the less rapid and determinate was the deposition:—the fecula remained in part suspended in the liquid, giving it a green hue; but in time it invariably subsided, and the quantity appeared to be the same in all cases.

It is sometimes customary in the beating vat, when the precipitation does not proceed with vigour, to throw into the vat a little lime water, or some other precipitant, to assist the Indigo in subsiding; the effect of such additions was tried on a small scale, taking care to exclude the air during the immediate application of the re-agent.

The acids and carbonated alkalies caused an immediate extrication of carbonic acid gas from the liquid, but produced no precipitate.

The caustic alkalies and lime, on the contrary, produced a copious deposit, unattended with effervescence. The colour of the deposit was yellowish-white, if the air was quite excluded; but it became green and blue, upon the slightest contact with oxygen. Careful experiments, however, proved that the blue colour was only produced by the indigo attaching itself to the precipitate; for all of the indigogene, or vegetable matter convertible into indigo, remained suspended in the supernatant alkaline liquid. The precipitate was composed of a yellow extractive matter, to which I shall again advert presently.

The measurement of the absorption of oxygen during the transition from the colourless to the blue state, was the next object of experiment. It was easily put beyond a doubt, that such an absorption took place; but several trials to measure it failed, on account of the extrication of carbonic acid gas, which was always much greater than the oxygen absorbed.

I thought that boiling would have driven off all the carbonic acid; but I was astonished, on filling an eudiometer bottle with liquid which had been entirely purged of all its free gas by ebullition, to perceive that the moment the tube containing 100 measures of oxygen was connected with it, a brisk emission of carbonic acid (about 50 measures) took place, and confused the results.

At length, by adding a little caustic potash to absorb the carbonic acid which might be generated, the diminution of oxygen became apparent, and in a few minutes all the oxygen contained in the eudiometer tube disappeared, the tube being too small to supply enough for saturation.

As the employment of potash was on some accounts objectionable, I also tried and succeeded in another method of obviating the presence of carbonic acid.

In a glass balloon, furnished with a stopcock, a vacuum was made, into which the liquid was suddenly introduced by a connecting tube. By this means a great deal of gas was separated, and, by repeating the action of the air-pump, much of what remained was withdrawn.

To the vacuum above the liquid, pure oxygen was admitted from a mercurial gasometer, its quantity being measured; and the air in the balloon was further analyzed by withdrawing a small portion. The balloon was then agitated, and water from time to time admitted to replace the oxygen which had been absorbed. The residual gas was again analyzed, to find whether the carbonic acid or azote had altered in quantity during the experiment, and the weight of the oxygen absorbed was ascertained from that of the water which had entered the balloon.

Four experiments conducted in this manner, yielded the following results :—

	1 *Expt.*	2 *Expt.*	3 *Expt.*	4 *Expt.*
Weight of liquid grains,	8455	5500	5166	6478
Weight of oxygen absorbed,	2.00	0.82	0.84	1.22
Weight of Indigo collected,	6.8	2.9	3.9	4.9
Proportion of oxygen in the Indigo per cent.	29.4	28.2	21.5	25.0

Making an average of 26 per cent. of oxygen absorbed; a quantity nearly double what Crum supposes to exist in pure indigo, and an average between that which he ascribes to cerulin and phenicin. It is, however, little more than the half of what Thomson states in his analysis.

There is hardly any occasion to seek a nearer concurrence between experiments made in the manner just described on the liquid of the vats; because the indigo forms but a very small proportion of the matter held in solution therein, and it is very possible that the other ingredients may also absorb oxygen, especially when the putrid fermentation commences.

It is evident, however, that a further analysis of pure indigo in the dry way is a very desirable object. It seems to have escaped the attention of Gay-Lussac and Thenard, and latterly of Marcet and of Dr. Prout, in their Researches upon the Constituents of Vegetable Compounds.

It is worth while in this place to mention a fact observed in the course of these experiments, namely, that where a slight excess of potash is mixed with the vat liquid, the indigo formed remains in solution, and passes through the filter with ease, leaving the precipitated extractive matter behind. This blue solution will keep for any time, and does not deposit its indigo even in the open air; but as soon as the alkali is saturated by an acid, it immediately precipitates, leaving the liquid colourless. If the dose of potash be not sufficient to throw down all the yellow matter, the solution retains a green colour, and cloth dipped into it takes a green dye. When hung up to dry, however, the mixed action becomes evident; for the dye remains fixed in the lower part of the cloth, while the yellow, more perfectly dissolved, spreads with the liquid to the upper part which was not dyed. There is no reason to believe, in this case, that the air changed the colour from green to blue, although such may be true of the dyer's vat-liquid.

The quantity of indigo deposited *per se* from 1000 parts of yellow liquid of the specific gravity 1003.1, varied but little, and may be estimated at 0.75 parts. In practice, a vat of 637 cubic feet is considered to yield a good produce at 16 seers, which is as nearly as possible 0.75 per 1000 of liquid. The common produce of the vats in this part of the country does not exceed 0.5, or one five thousandth of their weight of indigo.

But when potash, lime, or other precipitants are employed, the weight of the indigo is much increased; not, as experiment proves, from an increase of the actual produce, nor from any union of the precipitant with the indigo, but from its causing the deposition of another matter, to which I have given the name of yellow or brown extract.

To obtain this yellow extract in an insulated state, 10,000 grains of mother liquid were evaporated to dryness: a solid residue was in one case collected, weighing 47 grains. With another liquor it was 20 grains heavier; and in the experiment made on a small scale with the leaves, where the fermentation continued for 24 hours, the same weight of mother liquor yielded 246 grains of residue. As this element of the plant seems to be so variable, and as it must evidently produce much influence in the manufacture, it is probably one of the chief causes of the nicety usual in timing the fermentation, and of the variable tendency of the indigo to precipitate in the beating vat. The dried extract has a dark-brown colour and vitreous surface, similar to that of dried gluten, or the brown extract of toast-water: it has a peculiar, not unpleasant smell, and rather a bitter taste: it deliquesces in a damp atmosphere, and dissolves in water, which it dyes of a deep brown or bistre. Although the original mother liquor is exceedingly liable to ferment and become putrid, the brown matter undergoes no change, either when kept dry, or in a moistened state.

It is precipitated from its aqueous solution by potash, soda, ammonia, lime, and their carbonates; by infusion of galls, acetate of lead, and nitrate of silver. The acids, and prussiate of potash, did not affect it; but the action of re-agents was not investigated in detail.

Sufficient has been adduced to prove, that whenever lime or the alkalies are used in the vat, the indigo must be adulterated, more or less, with this substance; and I suspect that the brown and green matters, separated by Chevreul and others, in their analysis of indigo, are attributable to this source. In an experimental way, indigo of a true dark green colour was easily collected, which weighed more than twice as much as the blue colour alone: it was also harder and more compact, and more liable to shrink and crack than the pure indigo cake; for besides the impurity just described, there was always found a large proportion of earthy residue, on burning indigo, where a precipitant was used, than where it was not; the quantity was even as much as double or triple in amount.

Carbonate of lime, alumine, and oxide of iron, are the chief ingredients of the earthy residue, and I have seen them vary from five to nearly fifty per cent. of the indigo: the last, as may be supposed, was merely refuse, and quite unsaleable.

Carbonate of lime is seldom absent; for whatever lime may be in the plant, or in the water, is sure to be precipitated by the carbonic acid developed during the fermentation. Some manufacturers are in the habit of employing rain water purposely to avoid this source of adulteration, but it is doubtful whether the plant will not itself bring a portion of earths into solution.

Upon the whole, it may be safely decided, that the purer the water is, and the cleaner and the more simply all the operations of the manufacture are conducted, the more beautiful and rich in colour will be the indigo: and it should be the first maxim of the planter, that it is purity, and not weight, which gives the value to his produce in the market.

In conclusion of the present desultory notice, which will, I hope, at least serve the purpose of leading others to bestow further attention upon the subject, I annex an analysis of a specimen of indigo, denominated fine blue in the Calcutta market, made in the year 1820, to which I have referred in a former part of this paper.

ANALYSIS OF CALCUTTA INDIGO.

100 grains heated white in a closed platina crucible left a porous grey carbonaceous mass, with metallic lustre, weighing . .	49.0
Burnt with access of air, the 49 grains were reduced to . . .	7.42

1.—*Examination of Earthy Residue*, 7.42.

1. Boiled dry in nitric acid, and then digested in muriatic acid, a brown residue of oxide of iron and alumine remained .	2.7	
2. From the solution, ammonia threw down alumine . .	0.75	
——————— Oxalate of ammonia—lime equal to .	0.9	
3. The clear liquor evaporated left red oxide of iron . .	3.05	
		7.4

2.—*In the humid way.*

1. 100 grains of indigo digested in boiling water, some green and dark brown matter was dissolved, which, when dry, weighed .	1.6
2. Alcohol boiled over the remainder became of a bright claret colour, and yielded, on evaporation, a dark brown matter and a little yellow resin, weighing	2.0
3. Dilute muriatic acid then took up a mixture of greenish vegetable matter and earths, which were afterwards separated by burning. The green matter was thus found	7.2
4. The indigo now deemed pure weighed only	79.5

This sample of indigo, therefore, was of the following composition:

Oxide of iron	5.75
Alumine	0.75
Lime	0.90
Green vegetable matter	8.80
Red or brown ditto	2.00
Pure indigo	79.50
Loss	2.30
	100.00

Mr. Burnett *on the Functions and Structure of Plants: with reference to the Adumbrations of a Stomach in Vegetals.*

To the EDITOR of the QUARTERLY JOURNAL of SCIENCE.

DEAR SIR,

"*Was not the first animal that ever lived, a plant that found out the blessing of a stomach, and ran away with it?*" Such was a proposition suggested to me after the Physiological Lectures I gave at the Royal Institution last spring, in which the form and functions of the various organs of the vegetable frame, and the distinctive characters of the several grades of the organic realm, had formed the subject matter of discussion.

This was a hasty conclusion, and one certainly not warranted by the premises adduced; for although the stomach is confessedly a most important organ in the animal economy, an organ to which we scarcely find anything analogous in plants, and locomotion one of their most valuable endowments, still neither are constantly present; and even were they, sensation, true instinct, and volition would yet remain, the best diagnostics of the animal creation. But there is something peculiarly terse and epigrammatic in the idea which would thus boldly, and in so few words, enunciate two of the most celebrated definitions of a plant, and include their necessary interdependence; for physiologists have long dwelt on the circumstance, that "vegetables are nourished by their external, while animals are nourished by their internal surface;" and naturalists have frequently referred to the power of "moving from one place to another" as the *most distinguishing characteristic* between brutes and plants. Age after age have these observations been repeated and re-echoed in almost every diversity of form;—thus Aristotle designated plants, *animals turned inside out*, while Jungius, Ludwig, Ray, and others, ringing their successive changes in scarcely differing words, define plants to be "organic bodies fixed to a certain place whence they are nourished and encreased;" or, as Boerhaave and Martin state the question, "adhering to another body in such a manner as to draw from it their nutriment:" and hence Linnæus, as the Stagirite had done before him, described a plant, and not inaptly, as *an inverted animal*. These definitions, though at first sight plausible, are too superficial, admit too many exceptions, and are clouded by too many obscurities, for the rule ever to have been made absolute, even as a popular test; and once gave rise to a repetition of that practical sophism, which it is said was long since enacted with regard to Plato's famous definition of a man, viz. "that he is an unfledged biped;" upon which the Cynic philosopher, having plucked a fowl, is reported to have exclaimed, "Behold Plato's man!" And thus the simple sacculate polypes have been turned inside out, and then the question asked, whether such an inverted animal had become a plant.

Continual reference we find both is and has been made, to the roots and absorbents of the vegetable body being external, *i. e.* distributed without itself, for plants even when vagrant as the Lemna of our ponds, and the Confervæ of our lakes and streams, still are in communion with their peculiar site, be it earth, air, or water; *i. e.* as Link observes, "deriving nourishment from the soil in which they grow."

Many physiologists hence would explicate the problem, why locomotion is so commonly the privilege of animals, and as necessarily, in general, denied to plants. For as life is alone sustained by the constant reparation of that machine which its actions as unremittingly impair, those beings which depend for this uninterrupted renovation upon supplies ever situate without themselves, must consequently be held in uninterrupted connexion with their external food—that is, with the soil in which they grow; whilst those which can intuscept their food (upon which they in like manner grow) are unrestrained to especial place, and may be truly said to bear about their soil within them.

Thus the earth has frequently been referred to as the common stomach of plants, and the decompositions which therein take place have been loosely likened to digestion; the roots of the vegetable body being considered analogous to the lacteal ducts of animals, for these absorb and convey as chyle, the eliminated extract of the one, as those imbibe and transmit, as sap, the juices furnished by the other. But the earth is not the external stomach of plants any more, nay, not so much as the nest is the external uterus of birds: the analogy is greatly forced; and the parallel would have been much closer, had the earth been described as the kitchen, rather than as the stomach of vegetables; fermentation, putrefaction, and the other changes which therein go on, being much more similar to the processes of cookery, than to the several stages of digestion. In truth, the lymph absorbed by the roots is still crude aliment, nor does it become changed into proper sap until after it has been elaborated in the leaves or other assimilating organs of the vegetable frame; the digestive, circulating, and respiratory functions being all of them more or less combined in plants, and never so separate in their organic systems as in brutes and men.

My present object, however, is not to shew the insufficiency either of locomotion or a recipient stomach, as diagnoses between animals and plants; for *Conferva*, *Volvox*, the *Corallines*, &c. have long certified that point; but rather to trace the slight adumbrations of a stomach that we find in plants; an organ so all-important, all-engrossing to animals at large, that to cater for its due supplies their other members are so chiefly, so constantly employed, that one might almost be disposed to think, that they do not "eat to live, but live to eat:" and which anticipations, though faint, are curious; as far as I know, they have not hitherto been dwelt on, or at any rate would seem too slightly to have been passed over.

Nature, ever fond of working on the same model, seldom arrives at any new form suddenly or *per saltem;* but in her progress towards perfecting a type, by scarcely perceptible degrees modifies the most diverse organs, prefiguring, for many successive grades, a change, which, subordinate when first perceived, subsequently becomes of paramount importance. Every function, or system of functions, upon the due performance of which vitality depends, will equally attest this fact, and with equal truth; for whether we examine the nutritive or the reproductive apparatus, the digestive or the respiratory systems, the organs of sense and motion, or the phenomena of irritability and instinct, each will witness, as each will furnish the examples, that the progressive rise of organic beings, whether animals or plants, consists rather in the separation and perfection of the respective functions, and the organs which they severally possess, than in many distinct endowments allotted to their various grades—rather in the perfecting those principles at first laid down, principles common to all, and by the enjoyment of which all subsist, than in any new mode of being peculiar to different kinds. The modifications of these general principles are numerous and vast; still the same plan is evident, the same rule governs, the same principle prevails; and as in one so in all, the self same dogmas uniformly guide, and every where direct the whole. Thus the humble moss and fungus, the lowly conferva, the most simple protophytes, differ not so much from the giant oak, the lordly palm, or the irritable mimosa, in what they do, as in the manner their essential duties are performed: similar ends are achieved by each, though diverse instruments are frequently employed. They are each endowed with organs of nutrition, by which they support themselves; they are each possessed of organs of reproduction, by which they perpetuate their kind; but in the one the absorbents pervade the whole structure, or overspread the entire surface, so that every part is equally and indistinguishably root: and again, if torn into many pieces all the parts will grow, for all alike are germinating points, each having the essential powers present in every part; while in the other series the nutritive system is more or less separated from the reproductive, the one exhibiting a proper root, the other a complex flower and seed. This separability of function, and distinction of structure, for the more or less perfect evolution of the various organs, and concentrating the power and energy of each, is a subject well worthy consideration: the grades are not less curious in animals than in plants; nor more so in the one than in the other: let an example from either reign suffice for illustrations.

Take the freshwater polype (Hydra viridis or grisea) a small lump of translucent jelly, about the size of a pea when contracted, but when extended, and viewed under favourable circumstances, lengthened in its body to about three-quarters of an inch, and more resembling the finger of a small glove, with a few ravelings round the edge, than any other familiar figure. This creature possesses neither wings nor legs, nor any of the ordinary organs of progression; it is apparently homogeneous in its structure, shewing not even a rudiment of bone for leverage, or a semblance of muscle for contraction, and yet it protrudes and withdraws its tentacles, moves from leaf to leaf, travels from plant to plant, from stone to stone, quits the dark and approaches the light side of the vessel in which it may be kept, basks in the sun-beams, enjoys the warmth of summer, becomes torpid during cold weather, and hybernates like the tortoise or the dormouse; retreats if touched, defends itself when attacked, and often attacks in turn; pursues its prey with avidity, and, although it has neither tongue, nor teeth, nor palate, yet with hungry relish it devours the minute animalcules it can catch; nay, even with cannibal propensities, will force smaller or weaker individuals of its own species into its simple pouch or stomach, digest a part, and then reject the fæces by that single aperture which is both entrance and exit, both mouth and vent to this gastric prototype, which thus absorbs a part of its ingested food and vomits up the rest; such being the natural process in this simple being, to which the higher grades return in many cases of disorder or disease. And yet so finely does this *prima communis via* participate in the peculiarities of digestion, and acknowledge its general laws, that, like the animal stomach of the highest grade which will digest a bone when dead, but cannot act on a pulpy worm when living, this pouch can only feed on prey that has been truly killed. Trembley, I think, it was who observed two hungry polypes fighting which should become the other's meal; or perhaps the little one endeavouring to escape, the greater attempting to devour the less: strength, however, at last prevailed, and this Saturnian polype swallowed at one gulp his son: the little fellow, not being, however, slain, was indigestible, and played such freaks within his living tomb, that the greater one, quite sick at heart, returned his dinner, unhurt, uninjured, to the light of day. But again, the polype has neither eyes nor ears, nor any of the ordinary organs of our senses, and yet it sees and feels, or at least is sensible both to light and touch, and probably to odours and to sound. Every part of this thing's body is equally sensible to the various stimuli which affect its system; it is an eye, an ear all over, but of what a kind!—an eye which sees not, an ear which does not understand: and when vision is to be perfected, the visive function becomes isolated, and the power concentered to a peculiar organ, which is developed by degrees to its highest point; and as of the eye, so of the ear, the hand, and all the rest.

In like manner, as among animals, so also among plants, a division of labour, and separation of systems, leads to the perfecting of organic structure, and of functional achievements. The Tremellæ and Oscillatoriæ are closely analogous in many respects to the polypes just referred to, and hence may form a parallel illustration. These simple gelatinous existences, known to the vulgar as "fallen stars," or sleeping "Wills o' the Wisp," are so ambiguous in their simplicity, that the learned doubt whether they should esteem them animals or plants; but as they possess not locomotion, nor exhibit any of the pugnacious and other qualities of the polype, which in higher animals are known to be the offspring of volition, sense, and instinct; and as the doctrine of irritability will sufficiently explain all the phenomena which they evince, it is more philosophical to consider them as plants. These slimy or gelatinous Tremellæ nourish themselves, as seems proved by their assimilated increase of size; they reproduce their kind as seems witnessed by their consecutive duration; yet they have nought that can be truly called root, or branch, or leaf, or flower, or seed. Many of the Confervæ are vagrant, unattached to any spot, and they, as well as the generality of the Fuci, even when fixed to cliffs, or rocks, or shells, are so adherent, or rather adherent to such substances, that the one can afford, the other derive, no nutriment through what would seem their root, but absorb their food by the whole of their frondescent structure.

The Fucus natans, the Conferva vagabunda, C. Ægagropila, &c. are not only decidedly nourished thus, but are also unattached and wandering plants, vegetable *vagabonds* as their names import, being apparently destitute of root; while the Tuber cibarium, or truffle, on the other hand, would seem to be wholly root, it has neither stem, nor leaf, nor flower, nor fruit; and yet as the former are nourished without a root, so in the latter the functions both of respiration and reproduction are essentially and effectually performed without even the semblance of those organs which are elsewhere their peculiar sites. Thus, in one type, *e. g.*, the Tremellæ (nostoc), we have nought distinct of any organ, or root, or stem, or leaf, or flower, or seed; in another, as Tuber (cibarium), they are all combined under the likeness of a root; in the Confervæ and Fuci sometimes the similitude of leaf, sometimes of stalk, would seem chiefly to prevail. In Testudinaria, the intermediate caudex (in most plants so obscure) predominates; while in Aphyteia and Rafflesia little else, save flower, is known. In the rushes and the cacti, the leaves are latent in the stem; in the lichens and the ferns, the reproductive germs are hidden in, or united with, the leaf-like fronds; while in the more common and most developed plants, the root, the stem, the leaf, the flower, the seed, are all distinct; the root absorbs, the stem upholds, the leaves respire, in short, each segregated organ is found fulfilling its especial duty; and neither performing, nor fitted to perform, the function of another: and yet in some, an occasional relapse is made in one or more particulars towards that primitive universality of system, whence, after repeated efforts, nature has extricated her superior works; which relapses are the intermediate links that establish an uninterrupted connexion from the highest to the lowest grades. Thus floral germs will grow, as in proliferous flowers, in continuation with the parent stem, returning to the character of buds; and gems occasionally will loosen, and assume, in part, the functions proper to seeds. Leaves and stalks will radicate at many points, and by art be separable, or often separate spontaneously, into many independent plants; or, even without this violent discontinuity ensuing, absorbents will detrude themselves on the branches and trunks, even of forest trees, or pervade and cover an entire plant, so as to remind one of the primitive ubiquity of root; when, as in the protophytes, the nutritive and reproductive organs were mutually and indistinguishably blended with each other, forming but one common yet effective whole.

It is matter of notoriety that plants, even what we consider the more perfect kinds, derive nourishment from the atmosphere as well as from the soil; and this in very different degrees: that some depend much on this, some much on that; and again, in others it is directly the reverse; that some plants will flourish on very poor soils, and others wane in comparatively good ones; that thistles will grow and enrich a plot where corn would degenerate and die. The turnep husbandry has practically familiarized this to every one, and the farmer well esteems the rest, to say the least of it, if not improvement, which such a crop affords to land, while wheat impoverishes it greatly. Experiments have proved that plants, with few and small leaves, depend almost entirely on the resources of the soil, those with many and large ones almost as exclusively on the atmosphere around them; the mosses, and most of the pseudo-parasites, for example: and some of the Epidendra, as the Flos aëris, will grow while hanging from the boughs of trees into which no roots are sent, or even, if suspended by a cord from the cieling of a room, will produce leaves and flowers for months or years together, nourished only by the elastics floating in the air. In some of the simpler tribes, the stem or entire plant, and in others, both sides of the leaves, or leafy appendages, equally absorb nutriment from the water or the air; others absorb chiefly or only by a single surface, which sometimes is the under, sometimes the upper page; and when, as in the connate leaves of Dipsacus, the hollowed basin of Hydrocotyle, or the follicular appendages of some rare plants in which a sac is formed by the intorsion and coadunation of their leaves, in which water and other matters are not only lodged and retained, but can be thence absorbed by the vessels of the plants which are spread therein, a considerable advance has been made in these pouch-like reservoirs towards an internal cavity for the reception and retention of food.

In many plants, in which concavities are found of various shapes and sizes fitted for the retention of water when caught from rain or condensed from dew, the cups or sacs would seem to be fortuitous appendages; or rather anticipations of 'coming events which thus cast their shadows before;' for the water, when caught, seems not necessarily applied to any important use in the internal economy of the plant, which still depends on its terrene root for its support. In other instances, however, (in perhaps the Teasel, and certainly the Pitcher plants) the reverse is found to be the case, and a great advance is here perceived towards a digestive receptacle, over those simple plants which not only are destitute of such foliar appendages and sacs, but also are devoid of leaves; or even over those which having leaves, still are chiefly dependent for support upon their ordinary roots. These various examples which mark the progressive stages of developement in the organization, and the gradual separability of structure in perfecting the functions, even of automatic life, although unworthy a more formal essay, may probably be thought not undeserving this transient notice.

In Nepenthes and Sarracennia, previously referred to, this shadowing forth of an animal organ is carried to the furthest developement that occurs among plants; in these and other similar vegetables some of the leaves are of the ordinary types, while others are so congenitally incurved and connected at the edges as to form a pouch or pitcher of considerable extent, or the membranous sac is even an especial appendage separate from, although a continuation of, the midrib and proper expansion of the leaf. In many instances this apparatus is fitted with a lip or lid, by which the mouth can be shut and opened; the machinery of which limb is so contrived that, when the cavity within is well supplied, it closes to prevent evaporation; that when the stock is diminished or consumed, the lip is raised, so that the mouth again is open to receive the falling rain, or the rising dew. That such plants condense into their receptacles large proportions of atmospheric moisture, as other plants do on their general surface, common observation will sufficiently attest, for they continue well supplied, even in conservatories, where no rain can reach them; and in many, the over-hanging lid forbids the entrance of falling moisture, but its arched form is well fitted to receive and condense the rising vapour: and it has been noticed that these pouches become fuller of liquid after the neighbouring plants have been watered, or the floor of the hothouse has been wetted, although none has been supplied to those in question; and the inner sides of these cavities being downy, or often thickly set with hairs, are thus by their structure well fitted to condense the aqueous vapours which float around them; and the throats of those which cannot closely shut their mouths are frequently so contracted, as, in like manner, to retard the evaporation of the water they contain. Still these plants, although thus curiously constructed, depend greatly also on the soil for their support; and hence, their stems are unlike the stems of the attached polypes, (Madrepora, Millepora, &c.) which rather resemble the Fuci, in that the organ of their attachment is not the organ of nutrition, for, although adherent to a certain spot, the stem by which they are fixed is a mere fulcrum, and not in anywise a *root;* their absorbents are confined to their upper members, and, as is probably the case, with many Hydrophytes, chiefly disposed on one surface only. In some of the simplest monads, in which no cavity can be traced, the outer surface must be the seat of absorption, and one great effort, in animalization, is the hollowing such a gelatinous point, so as to form a cavity which may represent a stomach; many animals there are, as the Hydatids, which are wholly or little else than stomach. In the polype this pouch, simple as it seems, is, notwithstanding, considerably advanced; still it is a digestive system only, for the respiratory, circulating, sentient, and reproductive, are all in embryo; the entire surface is both skin and lungs; there is neither heart nor vessels, nothing like a brain or nerve, never an organ, or for sense, for instinct, or volition: and even if cut into twenty or fifty pieces, each will become an entire being, possessed of all the faculties, and practising all the arts which signalised its adult parent of which it was so minute a part. This indestructible divisibility of many of the lower animals which thus propagate by buds, by cuttings, or by offsets, was familiar to the vulgar long before the matter was dreamt of by philosophers; and when Trembley was experimenting on the Asterias and other radiate animals, the fishermen, who saw him cutting them in pieces, jocosely observed, "*Il perd son temps, il ne peut pas tuer ces choses.*"

Just so, the entire frond at first absorbs nourishment in plants, afterwards particular parts only; in some advanced instances the root chiefly, in others the leaves also; in some both pages of the leaves, in others one side only; and in some only particular parts even of the selected surface. In most, where the leaves are feeders, they form organs of absorption only, but in some few cases they become organs for the retention of food also. And here another circumstance requires our attention, for in these receptacles, especially of Sarracennia and Nepenthes, are generally (almost invariably) found flies and many small insects, which tempted to enter, either by the fluid itself or the excretions from the plant, often of a saccharine nature, with which it becomes mixed, are denied egress, either by the closing lid, its vaulted form, the contracted throat, or the bristly barrier before alluded to; the hairs of which being all pointed inwards, like the entrance to an eel-weir, or the wires of a mouse-trap, may easily be passed in one direction, but not in the other. The prey is thus entrapped and held, just as by the teeth of fish and other animals, frequently situated, not only on the tongue and palate, but also in the throat and stomach, being, like the hairs in these plants, organs of retention, not of mastication for their food.

It has often been objected to as an act of cruel amusement, if not of sheer malevolence, on the part of Nature, to set these vegetable fly-traps, as in the Dionæa, Drosera, Sarracennia, &c., to ensnare and destroy the heedless flies, shortening their already ephemeral existence; but observation and experiment would rather lead to the conclusion, that such sacrifices of the smaller insects form no unimportant items in the food of certain plants. In the pouch of one small Sarracennia, examined a few days ago, I found twelve common flies, and two woodlice; and the multitudes imprisoned and destroyed by the Dionæa and other plants would lead one to believe, were it from their number only, that Nature could never sanction such a waste of animal life, were it not to answer some important end in the well-being of those plants she has furnished with these organs of destruction. The little sundew which grows commonly in our bogs and marshes, abundantly, near London, on Hampstead Heath, would itself furnish sufficient proof. I have often seen several flies or worms in the possession of one of these small plants, which was flourishing by its prowess, and fattening on the delicates it had caught. But in the Sarracennia the number is still greater, often, in the larger plants, so great as to cast a strong and offensive effluvium around. The decomposition, however, is a necessary process; and it is, probably, both modified and checked by the saccharine secretions of the plants, which, like the gastric fluids of the animal stomach, may be fitted, not only to digest, but also to retard or regulate the putrefaction of the ingested food. To pursue the parallel, Rumphius has observed that, within these pouches, a certain small squilla or shrimp, with a protuberant back, is sometimes met with which lives there; so that even this simple digestive apparatus is not free from intestinal worms. Other more serious diseases would seem likewise not unfrequently to prevail, for the discoloured spots in the pouches of Sarracennia indicate serious disorganization, and the powerful and rapid decomposition of food, when taken in too great abundance, may fancifully be likened to indigestion from repletion, and then the occasional offensive odours may perhaps be symptomatic of vegetable dyspepsia.

The water in these receptacles, impregnated by the half-decomposing animal matter, doubtless affords a highly-nutritive and invigorating diet to the plant, for it is well-known that the drainings of dunghills give a very powerful stimulus to vegetables, as the rainwater that percolates there-through dissolves and carries with it, in solution, much of the nutritious and more subtle ingredients of manure; and as the food of plants is chiefly, if not wholly, absorbed in a fluid state, the more soluble manures are ever the most conducive to their growth. Nor must the nitrogen thus afforded to the prehensile plants be overlooked in the account, when we know how potent an excitant ammonia is to the vegetable frame. These speculations would seem, in some measure, to admit of experimental proof, for the Sarraceniæ, if kept from the access of flies, are said to be less flourishing in their growth, than when each pouch is truly a sarcophagus; and further, I remember to have either heard or read of a physiological experiment made on two plants of Dionæa Muscipula, selected for this purpose, of nearly equal size and health; both were kept under similar circumstances, save that the one was restrained from flies, worms, and all kinds of animal food, while the other was daily fed with small strips of rump steaks; the result of which experiment was, that the Epicurean plant languished on its lenten diet, whilst the beefeater flourished on its more substantial fare.

The plants to which I have alluded being furnished, some with organs for the prehension, and others with pouches for the retention of matters that are or may be used as sustenance, perhaps approach the nearest in their structure to that involution and appropriation of a part of their surface, for the especial purpose of retaining food, and absorbing thence its nutritious particles, of any that we at present know; and, consequently, are the nearest approaches, the strongest adumbrations (slight as they are) that we meet with in the vegetable kingdom, of an organ so prevalent among animals as to have been thought their peculiar characteristic, viz. a *stomach*. The gradations of this prime nutritive apparatus, both in brutes and plants, is curious, from the general use of the entire surface to the special ordination of a particular organ, which organ becomes progressively more or less involved, as it is more or less essential and important; as in the higher types almost all the systems, whether nutritive or reproductive, ordinarily are: and yet the original external cutaneous digester, even in man, the highest grade of all, has not lost entirely the original community of function that signalized it in the lowest types; for it not only perspires and absorbs matters hurtful and salutary, but likewise forms carbonic acid with the oxygen of the air, being in some slight degree an organ of respiration, an external lung, as it is also a vestige of the external kidney, stomach, and intestines.

On the peculiar properties and powers of vegetable digestion, whether the apparatus be spread over an entire surface, or collected into an especial organ, I have purposely avoided to dilate; and yet this is a subject of most curious interest, one to which hereafter I may probably, if time permit, return. Mirbel acutely noticed, that "Plants alone have a power of deriving nourishment, though not, indeed, exclusively, from inorganic matter, mere earths, salts, or airs, substances certainly incapable of serving as food for any animals, the latter only feeding on what is or has been organized matter, either of a vegetable or animal nature; so that it should seem to be the office of vegetable life alone, to transform dead matter into organized living bodies." This is a most philosophic and important observation; one which would further lead to the investigation of the problem, whether plants, in any case, *produce* those alkaline and earthy substances which are mediately or immediately peculiar to them, and whose metallic bases are chemical elements in our present state of knowledge; but the mere allusion to this extensive and disputed topic is almost without the scope of the present trifle, already stretched unduly in its length.

To conclude, hence perceive we that the definition of Aristotle, though incorrect, was, for the age in which it was adventured, one of extraordinary astuteness, when he said that "plants are animals turned inside out;" for Linnæus, who repeated this observation many centuries after, in nearly similar words, declared that they are in the organs by which many of their functions are performed, truly and strictly "inverted animals." Efforts, we have seen, are continually made in each successive stage to separate the vital functions, and concentrate their force in peculiar organs, for the better performance of their several duties: just as in mechanics, a pin is best made when the head is the work of one artist; the shaft of a second; the junction of each to the other the employment of a separate artificer; and sharpening the point the labour of an especial hand.

In reference to the proposition with which we set out, if we have found certain locomotive vegetables, as the Confervæ Ægagropila, Vagabunda, &c., that can figuratively be said to have run away; these vagrant plants have run away without a stomach: and if Nepenthes and Sarracennia can, in anywise, be said to have "found out the blessing of a stomach," they have not been able "to run away with it."

Yours, very truly,
GILBERT T. BURNETT.

November 10*th*, 1829,
22, *Great Marylebone-street.*

Streets of the Metropolis.

No person who has occasion to walk, or even ride along the streets of the metropolis, but has cause to complain of their bad, uneven, and often dangerous state. From the necessary excavations for water and gas pipes; for drains, ovens and cellars under the pavement; and for occasional repairs of the pavement itself, the streets, especially the leading ones, are ever in a state of disorder and confusion.

True it is, that such inconveniences must be submitted to in such a place as London. The alterations and improvements, both public and private, constantly going on, must necessarily keep the streets in a state of revolution. But why such works are not done in the best manner; and why the City has not the best engineers, as well as the best of everything else, is most unaccountable. In many cases, it is often observed, that improvements are begun before the projector is aware of the extent he may find it necessary to go, or before the mason or pavior is prepared with materials to complete the work. This is entirely for want of concert among those concerned in the affair, and for want, it would appear, of a competent superintendent to direct the operations. The greater part of this endless doing and undoing is caused by the imperfect and slovenly manner in which the excavations are filled up. The workmen's endeavour invariably seems to be, to make all good as expeditiously as possible; using any kind of material nearest at hand, whether it be fit or unfit; finishing by relaying the stone; and ramming the surface with ludicrous exertion of lungs and arms, till all is smooth and level. But in a short week or two, behold, the regularly paved place, instead of remaining what the workmen had called "a good job," is become a dangerous foot, or wheel trap; and then it must be pulled up again, to the annoyance of passengers, and at a further expense to the public!

To this improper and careless way of filling up these openings, is to be attributed the dangerous settling of the houses towards each other in narrow streets, and requiring those frightful shores to keep the walls upright. The foundations are disturbed by these repeatedly-opened trenches; and the heavy drays and waggons acting as pressers, shake the buildings from their position.

To avoid such danger and damage, and that the pavement should remain as durable and firmly level as the art and aim of the pavior can make them, it is, above all things, necessary that more labour be bestowed in filling up these openings. As no wise architect would build on loose ground, so neither should pavement of any kind, nor even a common road, be laid on a *substratum*, which is not sufficiently consolidated to bear whatever weights may have to pass thereon. To obtain this requisite compactness, it is necessary that the material which is dug out should be used or rejected, according as it is or is not capable of being rammed as firmly as possible. In doing this, the temperament of the material, whether sand, gravel, or other substantial earth, should be particularly attended to; it should be neither too wet, nor too dry; if just damp enough to knead together under the action of the rammer, it is in a proper state. Every shovelful thrown in should be subjected to the rammer, from the very bottom to the top; not a crevice or corner of the mass should escape. This operation carefully executed, as it may be, will ever after bear any kind of paving, gravel, or road metal, without sinking more than the surrounding natural bed of the earth.

An approach to this rule of management, and to prevent the irregular sinking of the pavior's work, has been done in Fleet-street; by placing a thick layer of broken stone below the pitching. This will certainly be effectual, where the stones are laid upon a firm bottom; but where laid on, or mixed with loose, unrammed earth, even this expensive expedient will be found insufficient.

In some other leading streets, where the Macadamizing system has not been introduced, the paving has been done with great exactness; regularly cut, nicely squared stones only have been used; and these, as usual, very smoothly rammed, and afterwards grouted with thin mortar to fill the interstices, and cement the whole together. But in such cases, the executor will be disappointed; he trusts too much to the accuracy of the workmanship in placing the stone, and to his powerful rank of rammers behind. The fact is, the rammers should be *before*, and not behind the men who place the stone. Pavement laid on a sufficiently solid foundation, needs very little after-ramming, if properly bedded in sand, and laterally connected. The labourer, in using his massive implement, directs all his attention and powers to produce *regular smoothness*, instead of *durable firmness*. The stone which happens to lie too high, receives repeated blows of the rammer, while the very next, perhaps, which has been placed too deep, is scarcely touched. This, it is true, makes neat, but not durably firm work; for unless every individual stone in the surface receives *an equal share of the rammer's force*, the pavement will soon become irregular from the action of heavy carriages.

To obtain, therefore, a smooth and durably firm pavement, the bed of soil on which it is to be laid should first be put into proper form, and equally consolidated; and, if possible, of equal consistence throughout. A carriage road, whether paved with stones, or Macadamized, requires a solid, yet somewhat *depressible bed* to rest on: otherwise the surface material would be soon worn down by the grinding action of wheels. The surface material, which yields a little to pressure, lasts much longer than a road which reposes even on a solid rock.

A few words may be added respecting the watering of the streets; this, when done timeously and properly, has many advantages sufficiently obvious. But it is too frequently done carelessly: too much water is thrown on at once, which not only lays the dust, but is soon trodden into a deep covering of filthy mud: the watering carts then disappear till this mud is again dry, and flying in clouds through the streets, when it is again diluted into mud. This watering should be done properly, or not at all; and were the dust, when moderately moist, swept off and carted away immediately, it would save the scavenger much trouble and expense, and render unnecessary their laborious and bespattering scoop-work. M.

Analysis of a New Mineral, containing a hitherto unknown Earth, by Jacob Berzelius.

The mineral, of which the analysis is contained in the following paper, is found in syenite, upon the island Lov-on, lying off the coast of Brevig, in Norway. It was there discovered by Pastor Esmarck (son of Jens Esmarck, Professor of Mineralogy in the University of Xiania), who sent me a specimen of it for analysis, imagining, from its high specific gravity, that it might contain tantalum.

The mineral is black, without any signs of crystalline form or texture, and resembles entirely in external characters the gadolinite of Ytterby. Its surface is sometimes covered with a thin layer of a dark rust colour. It is very brittle, and full of cracks, on opening which the surface has a dull greasy lustre. On an entirely recent surface the fracture is glassy. It is heavy, having a specific gravity of 4.63. It is not very hard, being easily scratched by the knife. Its streak is greyish red. In powder, the mineral has a pale brownish red colour, becoming lighter the finer it is rubbed. Heated before the blowpipe, it loses its black colour, gives out water, and becomes nearly of the same colour as when reduced to powder, but does not melt. Heated to redness in an open tube, it gives off a very minute trace of fluoric acid.

With borax it melts easily, and if the mineral be in large quantity becomes opaque on cooling. No opacity is produced by *flaming*. The colour of the glass indicates the presence of iron. With saltpetre it shews also the presence of manganese. With phosphoric salt it melts, leaving a residue of silica, and the glass, which is coloured by iron, becomes opalescent on cooling. Here also, with saltpetre, the presence of manganese is indicated. With carbonate of soda the mineral is decomposed without melting, and leaves on the charcoal a yellow brown slag. By *reduction* *, if borax be added, small white metallic globules are obtained, which flatten under the hammer. They are lead, containing a trace of tin. On a plate of platinum, with carbonate of soda, the mass becomes green.

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* In the inner flame of the lamp.

The mineral seems to occur very sparingly. Professor Esmarck has informed me more lately that no new quantity has been obtained, as the closeness of the locality to the surface of the water prevents the rock from being blasted until the water be frozen.

The mineral contains a metallic body hitherto unknown, which, in its properties, belongs to the class which forms the so called *proper earths,* and its oxide is an earth which most nearly resembles zirconia, and, what is singular enough, possesses the greater part of the properties and characters found in the earth I formerly described, under the name of Thorina. This circumstance led me at first to imagine that thorina might possibly not have been, as my later experiments seemed to shew, simply a sub-phosphate of yttria, but a mixture of it with thorina. At the beginning of this investigation, therefore, I gave the name of thorina to the new earth,—and when afterwards, by a new analysis of what still remained of the mineral, in which I supposed myself to have found the *older* thorina[*], I could not detect a single trace of the *new,* I thought I could, with still greater propriety, retain for this last the same name, both because the former description for the most part agrees with it, and because the name has already been once introduced into the science. The new mineral itself I have consequently called thorite[†].

ANALYSIS OF THORITE.

a. 2.005 grammes in coarse powder were put into a small tube retort, connected by caoutchouc with a receiver, from which the gas given off was conducted through a tube filled with chloride of calcium. The loss, by heating to redness, was 0.1985. Of this, 0.19 was taken up by the receiver and the chloride of calcium, and consisted of water which gave a minute trace of fluoric acid. 0.0085 was gas given off.

The same portion, again heated to redness in a stream of hydrogen gas, from reddish brown changed to a blue grey, inclining to green, and lost again by the formation of water .03. Rubbed in the mortar, the mineral now gave a dark grey powder, very little acted upon by muriatic acid.

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[*] It appeared to me probable, that eudialyte from Greenland might contain thorina, since, at the time when Stromeyer analyzed that mineral, the properties of zirconia were not so well known as now, and therefore the new earth might be mistaken for zirconia; but I found in it, as Stromeyer has stated, only zirconia.

[†] Professor Esmarck calls it berzelite.—Tr.

b. Five grammes of thorite unheated and reduced to fine powder, being treated with muriatic acid, became yellow, and gave a slight odour of chlorine. By heat the evolution of chlorine became stronger, and the whole was gelatinized. Evaporated in a water-bath it left, after solution, .985 grammes of silica. This was dissolved afterwards by boiling in carbonate of soda; the solution diluted with boiling water, the clear liquor decanted, and the remainder boiled over again with carbonate of soda, there remained undissolved in the alkali small grains of quartz, a little powder of the stone which had escaped solution, and a light grey yellow powder which, by decantation, could be separated from the foregoing. This powder weighed .05 grammes; the heavier weighed .018, or together .07 grammes; after which there remained of undissolved pure silica .915 grammes. The grey yellow powder contained much silica in its composition, and, with carbonate of soda, before the blowpipe melted into a glass. I have not examined this more minutely.

The aqueous solution which had been separated from the silica, was precipitated with caustic ammonia, and the precipitate well washed with boiling water. The ammoniacal liquor which passed through was mixed with the evaporated washings, saturated with oxalic acid, and slightly heated, till the precipitate, which immediately appeared, had completely fallen. The precipitated oxalate of lime, burnt and treated with carbonate of ammonia, gave a slightly brownish carbonate of lime, weighing .241 gr. It was dissolved in muriatic acid, and the solution mixed, first with aqueous solution of bromine, and afterwards, in a corked flask, with very dilute caustic ammonia, till the acid was, in a minute degree, oversaturated. After twenty-four hours, there had separated from the solution, which, by degrees, became yellow, a quantity of oxide of manganese, which, heated to redness, weighed .01 gr. The true weight of the carbonate of lime, therefore, was .23 gr., answering to .1288, or 2.576 per cent. of pure lime.

c. The solution, remaining after precipitation by oxalic acid, was evaporated to dryness, and the sal ammoniac driven off by heat. The remainder washed with water left .018 grammes of magnesia, slightly tinged by oxide of manganese, which was in too small quantity, however, to admit of separation.

d. The washings by evaporation gave .0205 gr. of mixed chlorides of potassium and sodium. These were separated by chloride of platinum — the salts after mixture with it being dried, and the soda salt separated by alcohol. In this way the salt was found to contain .0113 chloride of potassium, and .0092 chloride of sodium, answering, the former to .007 gr. of potash, and the latter to .0049 gr. of soda.

e. The precipitate (from *b*) became dark by washing from the presence of protoxide of manganese. It was dissolved again, while still moist, in muriatic acid, and the filter carefully washed. Through the solution a stream of sulphuretted hydrogen gas was passed, which separated a black precipitate. From this precipitate well washed, hydro-sulphuret of ammonia separated a minute trace of sulphuret of tin, too small to be collected or weighed. The precipitate was treated with nitric acid to full oxidation—afterwards a little sulphurous acid was added, and the whole evaporated by a gentle heat till the excess of sulphuric acid was driven off. From the dry mass water separated a metallic salt, from which ammonia gave a precipitate in white flakes, which weighed .005 gr., and which before the blowpipe exhibited all the properties of the oxide of tin, and was reduced with carbonate of soda to white malleable globules. That which remained insoluble in water was sulphate of lead, and weighed .052 gr., answering to .04, or .8 per cent. of oxide of lead.

f. The liquid, after precipitation by sulphuretted hydrogen, was evaporated to dryness by a gentle heat. Towards the end it gelatinized, and left, after solution in water, .034 of silica. The solution was precipitated by caustic potash, which was added in excess, and the precipitate boiled with it. The alkali dissolved .003, which, at a red heat with *cobalt solution*, became blue without melting, and was, therefore, alumina. Neither this nor the alkaline solution contained any trace of phosphoric acid.

g. The precipitate thus treated with potash dissolved with ease in diluted muriatic acid, leaving peroxide of manganese, which, washed and heated to redness, weighed .081. There was found, also, a trace of oxide of iron, and of thorina, but too small to admit of separation.

h. The solution in muriatic acid was neutralized by caustic ammonia, and concentrated by evaporation, after which pure sulphate of potash was dissolved in it to complete saturation. There fell a fine white powdery precipitate, which was thrown upon the filter, and washed with a saturated solution of sulphate of potash. It was then dissolved from the filter by boiling water, which took it up without residue. The solution, precipitated by caustic potash, gave a white earth, which did not become yellow by washing, by which the absence of cerium was shown, and which, heated to redness, weighed 2.817 grammes, and was thorina, with a slight, yet low, tinge, from a trace of oxide of manganese, which could not be quantitively separated, but the presence of which was also shown by carbonate of soda on a slip of platinum. Its quantity is, at all events, too small to affect the weight of the earth, in any sensible degree. In further trials, made upon this earth, it was found free from all other mixture.

i. The solution, precipitated by sulphate of potash, was again precipitated by caustic potash, the precipitate well washed, and treated afterwards with carbonate of ammonia. The portion insoluble in carbonate of ammonia, heated to redness, weighed .1905. It was dissolved in muriatic acid, and decomposed, in the usual way, by succinate of ammonia into .162 peroxide of iron, and .0285 peroxide of manganese.

k. The solution in carbonate of ammonia was evaporated to dryness. The dry mass was digested with dilute acetic acid, which became of a yellow colour, and with caustic ammonia gave a beautiful bright yellow precipitate, which, after washing and heating to redness, became dark green, and weighed .079, proving to be protoxide of uranium.

l. The residue, after digestion with acetic acid, was yellow grey with muriatic acid : it gave a colourless solution, which was first mixed with tartaric acid, and afterwards supersaturated with ammonia, without the appearance of any precipitate. Sulphuretted hydrogen separated a minute portion of sulphuret of iron, which, dissolved in nitric acid, and precipitated by ammonia, gave .008 peroxide of iron.

m. The liquid precipitated by sulphuretted hydrogen was evaporated to dryness in a weighed platinum crucible, the sal ammoniac driven off, and the tartaric acid destroyed by burning, after which there remained .073 of a light yellowish earth, which was found to contain neither yttria nor titanic acid, and which, in all respects, comported itself like thorina, tinged with peroxide of manganese. I ought on this occasion to mention, that the presence of thorina in the solution after precipitation by sulphate of potash, is an error in the process, arising from the circumstance, that the earth was not fully precipitated by the sulphate of potash; an operation, however, which is very easy when the solution employed is not too concentrated. I shall return to this subject in describing its double salt.

The whole results of the analysis give for the composition of thorite :—

	in 5 grammes.	in 100.
Thorina *h*) 2.8175 + *k*) 0.079	2.8905	57.91
Lime *b*)	0.1288	2.58
Oxide of iron *i*) .162 + *l*) .008	0.1700	3.40
Oxide of Mang. *b*) .01 + *g*) .081 + *i*) .0285	0.1195	2.39
Magnesia *c*)	0.0180	0.36
Oxide of uranium *k*) .079 oxidule + .014 oxygen	0.0804	1.61
.. of lead *e*)	0.0400	0.80
.. of tin *e*)	0.0050	0.01
Silica *b*) 0.915 + *f*) 0.034	0.9490	18.98
Water *a*) $\frac{0.19 \times 5}{2}$	0.4750	9.50
Potash *e*)	0.0070	0.14
Soda *e*)	0.0049	0.10
Alumina *f*)	0.0030	0.06
Undissolved mineral *b*)	0.0700	1.70
Loss	0.0359	0.49
	5.	100.

Since, in this analysis, chlorine is developed during the solution of the stone, it is plain that both the iron and the manganese exist in it in the state of peroxide. From the investigation into the saturating power of thorina, hereafter detailed, it follows, that the oxygen, in all the bases together, is equal to that contained by the silica. That in the thorina is a little more than twice the oxygen in the other bases; but both the great number of the latter, and the mixture of bases having one, with others having three atoms oxygen, between which no multiple ratio can obtain, induce me to consider thorite as an accidental mixture of several hydrated silicates.

(To be continued.)

Notice on the Observations of the Comet of Encke during its appearance in 1828. By M. Struve.

[From Le Recueil des Actes de la Séance Publique de l'Académie Impériale des Sciences de St. Pétersbourg, 29 Dec. 1828.]

At the annual meeting of the Imperial Academy of Sciences at St. Petersburgh, December 29, 1828, a notice was communicated by M. Struve, on the Observations of the Comet of Encke, made in the observatory at Dorpat, in the autumn of 1828, with the great refracting telescope of that observatory.

The comet was first seen by M. Struve on the 16th of September, New Style, which was several days earlier than it was seen elsewhere. It then appeared as an extremely faint nebulous spot; but early in October, and at a time when all other observers complain, that, from its faintness, they were unable to make satisfactory observations, the superior light of the Dorpat telescope enabled M. Struve to commence a series of exact determinations of its place. The micrometer attached to the telescope,—the spider-threads of which are illuminated, whilst the field remains dark,—is also peculiarly suited to the observation of very faint objects.

M. Struve's series of observations commence on the 13th of October, and terminate on the 26th of December, comprising an interval of nearly two months and a half. By submitting the observations to the calculation of probabilities, he finds the probable error in the determinations of right ascension and declination, between the 26th of October and 10th of November, to be about two seconds of arc, and from the 30th of November to the 25th of December, to be less than one second.

An highly interesting and curious part of M. Struve's communication, is the remarks with which it concludes on the inferences which may be drawn from the observations in regard to the physical nature of the comet. We shall not attempt to shorten these remarks, lest we should do them injustice.

I. When first seen, the comet had a diameter of between two and three minutes; yet, from its extreme faintness, was scarcely visible. Now, as objects shining by their own light become invisible, not from increasing faintness, but from reduction of magnitude, it follows, that Encke's comet is probably a body shining only by the reflected light of the sun.

II. On the 29th of October, the comet covered a star of the 9—10th magnitude, which passed within 22 seconds of the most luminous (and therefore the densest) part of the comet, without losing any portion of its brilliancy. On the 7th of November, a star was seen so situated as to be at first mistaken for a nucleus shining through the nebulous matter of the comet, until its motion discovered its real nature. And on the same night another star was covered by the comet, and passed within a very few seconds of its most luminous part, without being in the least degree obscured. From these observations, it appears that the comet had no solid [opaque] nucleus.

III. In regard to the form and appearance of the comet, as early as the 7th of November, a nebulous spot, more luminous than the rest, could be distinguished, extending, as shewn by the plate, from α to K. (*Vide* Lithographic Plate, Fig. I. and II.) This spot was not simply a condensation of the nebulous matter towards k, the most luminous point, for it had a well-defined limit at α, whilst at K the limit was not so well defined as at other parts. On the 30th of November, the limit of the spot and the exterior limit of the comet coincided at α, and at this time there appeared to be at k a nucleus, with imperfectly defined limits, shining through the nebulous matter in which it was enveloped. It was this point k, the centre of the apparent nucleus, which was adopted throughout as the precise point to be observed in determining the place of the comet. The figures in the accompanying wood-cuts shew

Fig. III. 7th Dec.

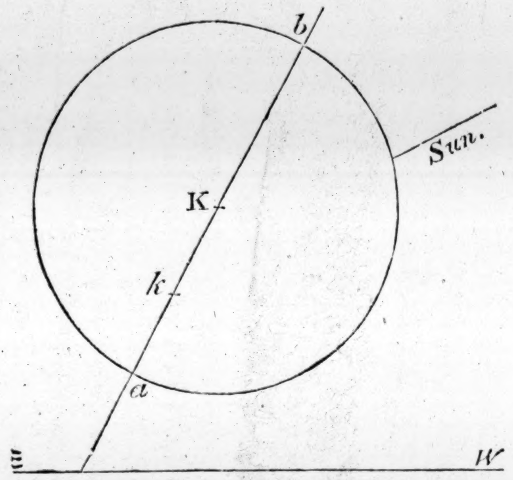
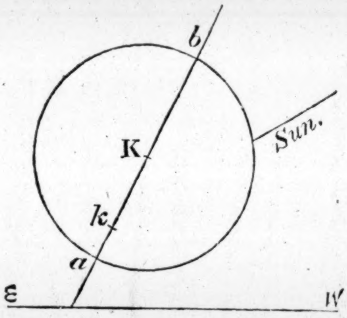


Fig. IV. 14th Dec.



the eccentricity of this point, as seen on the 7th and 14th of December. These two figures, from the first having been drawn under unfavourable circumstances, and the latter whilst the moon was shining, do not quite accord in shape with the correct representations in the lithographic plate.

IV. On the 7th of November, the whole nebulous mass had a diameter of eighteen minutes, and on the 30th of November it had lessened to nine minutes, although, during the interval, the comet had approached nearer to the earth, and although, from its increased vicinity to the sun, its light should have been twice as strong as on the 7th of November, and consequently its extent and limits more clearly visible. The fact of the contraction of the whole nebulous mass was further confirmed by the coincidence, on the 30th of November, of the limit of the whole mass with the boundary of the more luminous included spot; whereas, on the 7th of November, the boundary of the spot was considerably withinside that of the nebulous envelope. M. Struve is positive in the recollection of his surprise at the diminished size of the comet on the 30th of November, at which time he had expected to have found it much larger than on the 7th; particularly as, from its increased light, it had become visible to the naked eye as a star of the 6th magnitude.

V. If the line *a b*, which in all the figures passes through *k*, the most luminous point, and K the geometric centre of gravity of the whole figure, be called the axis of the comet, all that part which is on the side of *b* may be regarded as a tail occupying both sides of the axis. The best defined boundary was always on the side of *a*, and the least defined, on the side of *b*; the former taking a parabolic curve from *a* towards *c* and *d*. But the point most deserving of notice, is the direction of the axis in respect to the sun. Its apparent direction was determined four times on the 7th and 14th of December; and the apparent direction of the tail of an ordinary comet in opposition to the sun being calculated, differences amounting to 100°, 145°, 154°, and 149°, appeared between the observed and the calculated directions. Thus the direction of the diffused or spreading part of the comet was always turned rather towards than from the sun, offering, in this respect, an absolutely new phenomenon; the more remarkable, because the best defined part of the contour, both of the envelope and of the nucleus, were on the side from the sun. If we compare these observations with those which have been made on other comets, Encke's comet will appear to have something in its nature different from other comets, and to be, in some respects, opposed to them; insomuch that we might be disposed to name it *a negative comet*.

VI. The appearance of this comet, when seen in 1825, was, to the best of M. Struve's recollection, that of an almost planetary disc of uniform light. If this recollection, grounded on observations made with a 5 feet telescope of Troughton's, can with propriety be compared with the observations of 1828, the appearance offered by the comet on its last return, was essentially different from that which it presented on the former occasion; and if so, we may anticipate important results from future observations.

"Will," M. Struve concludes, "the direction of the tail of this comet towards the sun, and the diminution of its magnitude, on approaching that powerful luminary, justify a supposition, that the sun attracts to itself a portion of the mass of this comet, which being thus subjected to successive diminutions, will finally be absorbed altogether, and the comet cease to exist in a separate form? Or may the diminution in its size, as well as its different appearance in 1825 and 1828, be attributed to a concentration of the matter of which the comet is composed?

"The solution of these questions must be left to future observations."

Observations on Quinia, or Quinine, and the preparations of Cinchona, by J. Hancock, M. D.

Quinine is now almost universally commended, both in and out of the profession.—Is this to be considered an absolute proof of its transcendent excellence, or can it be accounted for on other principles?—We know, that amongst the Chinese the root ginseng, considered amongst us as a very simple and innocent drug, is equally or still more extravagantly esteemed than quinine is at this moment with us.—There is a fashion in physic, as in everything else, and that a very changeable one in respect to remedies.

Pharmaceutical, like other hypotheses deduced from false premises, are the sources of innumerable errors. Seguin, an eminent French chemist, finding that infusion of bark was precipitated by galls, concluded, that it could be no other than gelatine which constitutes the febrifuge principle of the bark. Whilst this absurd error remained undetected, i. e. of the bark containing gelatine, common glue was found so successful in the cure of intermittents, as scarcely ever to fail or to disappoint the practitioner!

On this subject, we are greatly indebted to Dr. Paris for his firmness; for he appears to be the only author who shews the least disposition to oppose the unbounded innovations of the pharmaceutical chemists of the day. "It is only necessary in this place," says he, in vol. ii. p. 165, "to caution the practitioner against the too hasty generalizations of the too sanguine chemist; it has already been observed that those vegetable remedies, whose value has been established by the sober experience of ages, consist of different principles of activity, or, at least, owe a modified power to the compound of their several ingredients." And in vol. i. p. 285, he adds, "Does it not, therefore, appear that certain elements exist in the composition of vegetable remedies, as furnished by nature, which, although *individually* inert, confer additional strength and impulse upon the principle of activity with which they are associated * ?"

So far, indeed, from depriving the bark of a large portion of its active principle, the most sensible physicians have advised the addition of other aromatic and astringent substances to heighten its powers. In proof of this, I shall quote further from Dr. Paris's work.

The most powerful minds are not exempt from extravagant errors—Sydenham pronounced that "to add anything to the bark argues either ignorance or craft." He did not, however, intend at the same time that anything should be abstracted from it.

"The most respectable testimony," says Dr. Paris, vol. i. p. 307, "may be adduced to demonstrate the great advantages which have arisen from the various combinations of this heroic remedy."—Sir George Baker has said, that "there is less of reason than of severity" in the above remark of Sydenham; for that it was found in the cure of the intermittent which he describes, that, according to circumstances, sometimes the Virginian snake-root, and in other cases myrrh, were added with propriety and advantage; and, according to the experience of several practitioners, a drachm of the rust of iron, and the same quantity of black pepper, added to each ounce of the bark, were the means of subduing the most inveterate agues.—

"* When the structure of vegetable remedies shall have been thoroughly examined upon this principle of combination, much medicinal obscurity will be removed, and probably pharmaceutical improvements of value suggested; at all events, it will teach a lesson of prudent caution to the pharmaceutic chemist; it will shew the danger of his removing this or that element from a vegetable compound, merely because he finds, upon its separation, that it is inert. I dwell the more upon this point, because I feel that there never was a period in the history of medicine, at which such a caution was more necessary; for while the poly-pharmacy of our ancestors has driven the physician of the present day into a simplicity of prescription that on many occasions abridges his powers and resources, the progress of chemical knowledge has diffused through the class of manufacturing chemists a bold spirit of adventure and empiricism,—a mischievous propensity to torture our best remedies, in order to concentrate or extract the parts which they consider to constitute their essential ingredients."

Hillary speaks of an epidemic intermittent at Barbadoes, in which "the bark was of no avail, unless combined with saline remedies, or some of the tonic bitters."

In a subsequent page, Dr. Paris adds, "I shall conclude the present section by shewing, that many of our most valuable vegetables owe their useful properties to the joint operation of the several *distinct* and different ingredients which enter into their composition. How many substances does nature produce in the vegetable kingdom, in which the permanent tonic quality of bitterness exerts its influence in union with the transient stimulating powers of an aromatic principle? *Peruvian bark* may be said to combine within itself the properties of *bitterness*, *astringency*, and *aroma*; a fact which suggested the probability of our being able to produce an artificial compound that might emulate the effects of cinchona, and to a certain extent the idea appears to have been realized; for we are told by Dr. Cullen, that he frequently succeeded in the cure of an intermittent by a combination of *oak bark* and *gentian*, when neither bitters nor astringent, separately, produced the least impression; and I am informed by Dr. Harrison, that in the Horncastle Dispensary, of which he was for many years physician, he never employed any other remedy for curing the ague of Lincolnshire than equal parts of *Bistorta* (astringent) and *Calamus aromaticus* (bitter and aromatic), neither of which plants, *individually*, ever produced the least benefit in such diseases. Berzelius attempted to produce a compound of this description, by adding to the bark of the *ash* some *tormentil root* and *ginger*; and he observes, that it acted as an excellent tonic, and that, according to the experiments of his friends, it seemed to cure quartan agues *. In the aromatic barks and woods, such as those of the *canella, orange peel, sassafras*, &c. the aromatic principle † is combined with a bitter ingredient; a union which proves of singular service in the formidable bowel complaints so common in tropical climates."

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* "Dr. Young's Medical Literature," 2d edit. p. 570.

† "The vegetable kingdom presents us with many natural compounds of this kind; several of which might be pressed into the service of medicine with much advantage. With respect to the number and variety of such substances, it must be confessed that our Pharmacopœia contains but a meagre bill of fare."

Here is a weight of evidence added to our *daily experience* in proof of the efficacy of astringents and aromatics, both as febrifuges and as general tonics.

Now it is vulgarly believed, and indeed we frequently hear it asserted by those who ought to know better, that the sulphate of quinine contains all the virtues of the bark. A very little reflection, however, must convince any unbiassed mind of the falsity of this opinion. It neither contains the astringent nor the aromatic principle; and a large portion of bitter extractive is also rejected with the infusion, in the process for the manufacture of sulphate of quinine. All these are essential to the entire powers of the bark, as we know from their tonic effect on the system. The *resino-extractive* matter was considered by Murray as the principle of the greatest activity of any the bark contains. "Astringents," says Dr. Paris, vol. i. p. 152, "are capable of acting as permanent stimulants, of curing intermitting fever, and of obviating states of general debility.' These principles are entirely thrown away in the process—and as to the invaluable antiseptic properties of the bark, the quinine contains nothing of it.

All this is evident from the methods taken for the preparation of quinine. Of those persons who assert, that the quinine contains all the virtues of the bark, very few, I believe, can have reflected upon the process employed in its preparation. The following one, from the Paris Pharmacopœia, will serve as an illustration. A tincture of the bark is first made with alcohol, which is evaporated to an extract. This extract is next washed with repeated portions of watery solution of potass, till it ceases to colour the water, and then with pure water to wash away the potass. This residuum then, divested of everything soluble in water, together with the agency of potass, is the meagre portion of the bark which remains, and which is now to be digested in an acidulated liquor (*acide hydrochlorique*). This liquor is next to be filtered, boiled on magnesia, filtered again, and evaporated to dryness. This residue is ultimately to be digested in alcohol, filtered, and the tincture evaporated, which, after this long and tedious operation, leaves the quinine, not much improved, I presume, in the essential properties of the bark *. (See the Pharm. Univ. 1828, tom. ii. p. 374.)

It is well known, on the other hand, that the simple infusion of the bark, taken freely, will cure intermittent fevers, although it is supposed to contain little or no quina or quinine, for without the acid it is considered nearly insoluble in watery fluids. This doubtless is the case; for I find that the *sulphate* of *quinine* is scarcely soluble in water alone, and, even in hot water, it is very imperfectly so; but on adding sulphuric acid, it dissolves instantly, like ice thrown into hot water. In spirituous menstrua, it is also very readily soluble.

All the world, however, agrees, that quinine is an absolute specific for intermittent fever: according to the old adage, "Every body says so, and it must be true!" This I consider a mere hue and cry, emanating from high authorities, founded rather on the estimation in which the Peruvian bark itself is justly held, than on patient investigation and a careful comparison of results. The simple infusion of quassia likewise cures intermittents, especially if taken warm.

That those alkaloids, the quinine, cinchonine, &c. do contain, in a certain degree, the febrifuge properties of the bark, I by no means intend to dispute; and therefore, in so far as this fact is well established, let us freely offer our meed of praise to those distinguished continental chemists for their valuable discovery, (*valeat quantum valere debet,*)—less, however, for procuring the virtues of the bark in a minute form, than for promulgating a means of eliciting an active principle, most easily available in aqueous fluids, which may be considered a very important point †. Experience and reflection, however, have abundantly taught me, that these properties are most extravagantly exaggerated.

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* We know that alcohol is the most effective solvent for nearly all of the active vegetable principles—or those capable of acting with energy on the animal economy,—(gum, albumen, &c. pertaining exclusively to a watery menstruum, are comparatively inert)—yet the *active principles* are, in different degrees, soluble in water also;—the substances acted on must, therefore, after repeated coctions, washings, exsiccations, &c. retain but very little of those constituents on which their natural and clinical powers depend—especially in a substance like the Cort. Cinchonæ, consisting of *several distinct elements*, all of which are more or less necessary to its full and proper action, and which, it is quite evident, are thrown away in the process for obtaining quinine.

† It is not to the French chemists, however, but to Dr. Duncan, of Edinburgh, I presume, that we owe the establishment of that peculiar principle or agent which is considered the more active one, in the bark of cinchona, a circumstance which seems now to be, in a great measure, overlooked, and for the reason, I suppose, that the same learned physician had not indulged in the affectation and quackery of applying every discovery or new principle, right or wrong, to some use as a medicinal agent!

I could adduce many instances in support of this fact, but I shall allude only to one or two here.

Mr. Chandler, of Essequibo, had fever whilst in George Town (March, 1827). It continued for seven days. On the intermission, (having been duly evacuated,) he began taking quinine in the dose of 3 grs. every hour. The fever returned with severity, after an intermission of about twelve hours, when he had taken not less than 35 grs. of quinine. This paroxysm lasted for six or eight hours. On the next intermission, he commenced the remedy, and took to the extent of about 40 grs. The fever again returned, with little less violence than before. Thus we may say he had one return after having taken 35 grs., and another after having taken 70 grs. This would cost, at the druggists in Demerara, about 24*s.*, equal to the expense of two pounds of the best bark, which would be sufficient to arrest the progress of a dozen regular intermittents! But it should be observed, that a fever of the continued form, as in this case, seldom returns, although no bark or other remedy should be employed as a preventive.

A year ago I had a fever, in all respects similar to the case here noticed: it continued seven or eight days. I took no quinine or bark in any form; and I had no return of fever. Yet, Mr. Chandler, though a gentleman of sterling good sense, attributed his recovery entirely to the quinine. This, however, only shews how men of the soundest judgment are liable to be imposed upon.

A neighbour, Mr. M'Lean, entering the room at the time I was making some notes on this affair, began relating to me his own case. Before having heard my opinion, he said, that in the fever he had lately passed through, he had tried the quinine largely, but got no relief from it; that he was ultimately obliged to have recourse to the bark powder; that he had no return of fever after commencing with this, and that he found his appetite wonderfully recovered, and his strength increased by it.

The quinine employed, in both those cases, was known to be genuine, from the great manufactory at Paris, and was received through one of the most respectable houses in London.

Besides the absurdities above alluded to, there are various others creeping in, and those, too, sanctioned by authority, or at least by modern writers on medicine and pharmacy.

A new formula for quinine has lately become fashionable, which serves to exemplify the rage for new remedies, however absurd they may be, if introduced under approved auspices. It consists of 4 or 6 grs. of quinine to an ounce of alcohol, so that to take a dose of quinine, it is necessary to swallow what is equivalent to a wine-glassful of proof spirit. In Rennie's Supplement to the "Pharmacopœias," (page 451,) 4 grs., which may be esteemed but a common dose, as now employed, are directed to the ounce of alcohol of specific gravity .847, although a few drops would answer the purpose of dissolving the quinine. This is probably adopted from the French codex, and certainly the *march of intellect* is rendered evident, by its having found its way so early into our dispensatories. It will be a great comfort and convenience to be able to get a good strong dram under the delicate name of *Tincture of Quinine.* We shall see if the college of physicians will adopt this valuable recipe in the next edition of their "Pharmacopœia."

Another of these prescriptions is, to dissolve 12 grs. of quinia in two pints of madeira or sherry. This, I suppose, may be that which is puffed off, under the name of the Tonic French Wine (see Rennie, p. 419) for training!

These new remedies having been got up, without the imputation of *empiricism*, and ostensibly as the pure results of pharmaceutic or chemical research, is the reason they have been so favourably regarded by the members of the medical faculty, who, indeed, have received them as a boon almost without examination. Some few of the profession, however, we find have paused, or have dared to make a stand, and allow the tide of popular clamour on the infallibility of quinine to roll past them. The judicious Dr. Paris, as already observed, is the foremost one of the very few who have evinced sufficient strength of mind to resist the overwhelming current.

It should be observed, too, that *alkalies*, or analogous substances,—as potass, ammonia, magnesia, lime, barytes, &c. are invariably directed to be employed, as essential in the preparation of our *new alkaloid* remedies; and how is it possible not to suppose that the alkalines employed must, in some measure, modify the results?

Piperine, procured after a similar manner from black-pepper, said to be febrifuge and poisonous in large doses, is regarded as possessing the essential properties of black-pepper. Common sense, however, if it had confidence to hold up its head in the presence of *affected chemical science*, would reject such absurd opinions,—as the said piperine is, as we are told, an *insipid* powder, and totally destitute of all the sensible properties of that most potent and pungent aromatic berry of which it is the pretended essence!

And with respect to *emetine*, it is now well known, as asserted by Mr. Brande five or six years ago (Man. Pharm.), that "it cannot be safely depended on as a substitute for ipecacuanha. Although, therefore, with a heap of other novelties, it has a place in the Paris Pharmacopœia, it seems very doubtful whether it will ever be much used in British practice."

There appears, moreover, to be less economy in the preparation of quinine than was at first announced. It was stated that 8 or 10 grs. would generally arrest an intermittent; but it has now got up to 100 grs. or more: to procure which, nearly a pound of bark is required.

It may be observed, too, that the new principle called kinic acid, or kinate of lime, is also disregarded and thrown away in the process of forming the quinine or cinchonine. This substance appears to possess much of the sensible properties of the bark. It is extracted by macerating the bark in cold water, from which, by rest, the kinate of lime separates in plates. The lime being separated by oxalic acid, the kinic acid is left pure. It is of an acerb, bitter taste.

What has been said of quinine, is applicable to cinchonine. I have made no distinction, wishing, for brevity sake, to be understood as including both under the former title, because we have reason to consider them both merely as modifications of one and the same principle, procured by the same process, and possessing similar medicinal properties.

Although it is plain, however, that quinine is far from containing all the active elements of the bark, still the French discovery will, if the foregoing views are correct, contribute to indicate a means of obtaining all its virtues in a light and not unpleasant form, which has long been a great desideratum in pharmaceutic chemistry; and such a preparation, I presume, will be found in the infusion hereafter proposed, with addition of sulphuric acid, &c.

Preliminarily, I may observe, that Murray, in his "Materia Medica," has the following passage:—"In those varieties of continued fever, connected with debility, as in typhus, cynanche maligna, confluent small-pox, &c., Peruvian bark has been regarded as a valuable remedy. It is difficult, however, to give it in such quantities as to obtain much sensible effect from it, as, from the weakened state of the organs of digestion, *it remains on the stomach unaltered*, and is liable to produce nausea and irritation." This points out the necessity of searching for a method of extracting the active principles, (not a single insulated element,) so as to administer them free from the inert woody fibre.

In alluding, also, to the difficulty often experienced in giving the bark in substance, Dr. Duncan says, "An important objection is, that some stomachs will not bear it, from the oppression and even vomiting which it excites; that it has long been a pharmaceutical problem to discover what menstruum extracts cinchona most completely; and that we must have recourse to direct experiment to determine the degree of action possessed by each menstruum upon it. With this view many experiments have been made, and by very able chemists. But most of them were performed when the science of chemistry was but in its infancy; and even at this time, that branch of it which relates to those substances is so little understood, that the results of the latest experiments are far from conclusive*."

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* "Edinburgh New Dispensatory," p. 113.

Mr. Brande, in the "Manual of Pharmacy," even recommends the sulphate of quinine to be taken with diluted sulphuric acid, 15 minims, (nearly 20 drops to the dose,) with each grain or two of quinine. He observes as follows:—"Sulphate of quinine has been exhibited in doses of from 1 to 5 grs., three or four times a day, though 8 grs. are thought by some to be equivalent to an ounce of bark. It appears to me most prudent to begin with small doses; and, if they answer the intended purpose, not to increase them; if not, they may be augmented till the disease feels the influence of the remedy; but in large doses this salt will be found to nauseate, to create weight and pain in the stomach, attended by thirst, a white tongue, a quickened and hardened pulse*, symptoms which of course should be watched." This does not well correspond with the characters of the *bark* as an *antispasmodic.* Our author continues—"It has appeared to me to agree best when administered with dilute sulphuric acid, provided this plan is not otherwise contra-indicated. The following formula may therefore be adopted:—

℞ Quinæ Sulphatis ℈j;
Conservæ Rosæ, q. s. ut fiat pilulæ xx.
quarum sumatur una vel duæ ter die cum hausta sequenti.
℞ Decoct. Cinchonæ ℥j, Acid Sulph. dilut. minims xv.
Syrup. Aurant. ʒj misce."—p. 63.

Here we observe, that the acid is ordered to be added to the decoction already prepared, merely as a useful adjunct, and not with a view to evolve the quinine in the decoction, which could be done, only by putting them together at first, or digesting the acid upon the bark prior to straining. From this recipe, however, of Mr. Brande, we derive a strong testimony in support of the propriety and advantage of the method hereafter proposed; for if, as stated by this distinguished professor, the acid is to be deemed the best addition to the bark from its own intrinsic value, how much more estimable and important should it be considered, when applied in such manner as to elicit and bring into action that latent principle, the quinia, in which the chief energy of the bark, as a febrifuge, is supposed to reside!

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* Its action will, doubtless, be somewhat varied in different constitutions. I have recently been told by a friend of mine, Mr. Edward Dawson, who has tried the quinine, both here and in Demarara, that its use has, with him, been invariably attended by headache and dizziness.

Dr. A. T. Thomson, in the "London Dispensatory," p. 246, takes occasion to notice the efficacy of the bark in low typhoid continued fevers, in various malignant and putrid disorders, and in pure typhus; but he conceives—"that the best effects will be produced by the bark, when its use, in pure typhus, is delayed till the skin becomes moist, the tongue in part cleaned, and the urine deposits a critical sediment. The best adjuncts in these cases are the diluted sulphuric, or the muriatic acids, and aromatics, particularly the tincture of *capsicum.*"

At page 737 of the same work, it is remarked of the infusion of the bark, that "the sulphuric acid destroys the bitterness of the infusion, but not its astringency; and adds considerably to its efficacy." Possibly it may destroy the bitterness of the infusion by being added at the end of the process, or it may only cover the taste and render it less perceptible; but when digested on the bark with hot water, if the acid be not in excess, or be added in sufficient quantity only to neutralize the alkaloid principle, it seems to me rather to augment than to diminish its bitterness.

It would even appear that a vague idea has long since been entertained regarding the utility of sulphuric acid as an adjunct to the preparations of bark.—"It (the bark) is used for obviating the dispositions to nervous and convulsive diseases; and some have great confidence in it, joined with the acid of vitriol, and notwithstanding the latter, a milk diet,—in cases of phthisis, scrophula, ill-conditioned ulcers, rickets, scurvy, and in states of convalescence."—*Lewis's Disp.*

"Il n'est pas rare que les fiévres intermittentes bilieuses du printemps et de l'été deviennent putrides; les malades tombent dans un grand affaissment, et sont couchés comme des automates; le pouls est à pein sensible; le sang tombe en dissolution:—sans le quinquina dans ces circonstance, la plupart mourroient. On le donne alors en decoction, plutot qu'en poudre, uni avec les purgatifs, et encore mieux avec l'acid vitriolique."—*Cours Elémentaire de Matière Medicale de M. Rochfort.* Paris, 1789.

With a view to the investigation of this important subject, I instituted some experiments in September, 1827. On the 7th,

℥j of bark* was infused in 8 oz. of hot water, with ʒii of diluted sulphuric acid. This had scarcely any sourness, or not more than water with half the proportion of acid. The infusion had, however, a strong taste and odour of the genuine cinchona. In a subsequent experiment *not the smallest separation or change* of appearance was caused by adding the sulphuric acid to the infusion previously filtered, nor, on the following day, had the least change ensued.

The infusion of caramacata (a bitter febrifuge bark of the Orinoko) also takes off the acid taste of the sulphuric acid; but I found, on adding it to the settled infusion, that a copious yellow precipitate was presently formed.

This would, therefore, seem to warrant the conclusion, that whilst a certain principle in the Peruvian bark (probably cinchonia) which combines with the acid, remains soluble in water; a part, in the caramacata infusion, which combines with the acid, is insoluble in watery menstrua, and is precipitated.—This may be the active principle of the latter bark.

The cinchona infusion remained covered, and exhibited little or no change on the 20th. This suggested the idea that the acid kept it longer, and led to some experiments, which were begun on the 23d; and the results were, that the infusion of the bark alone became fœtid in a few days, the cinchonic odour was lost, and it had no taste of the bark. Others, with addition of vitriolic acid, retained their taste and odour, even for months afterwards. In all of them, however, a fungus or mouldy flake formed on the surface.—Those with the addition of bueyari, ginger, liquorice, together with the acid, remained least changed. That with lemon-peel was more acescent, owing probably to the mucilage of the peel. The liquorice covers the acerbity of the infusion and retards decomposition.

As to the respective values of the two kinds of bark in common use, the pale and yellow,—although the pale is said to contain little or no quinine, it appears to be by far the most efficacious medicine, when genuine, or when found to possess the true cinchonic aroma, bitterness, and astringency; for there are very different qualities of bark met with under this name, it being (although referred to *C. lancifolia*) derived indifferently from several other species*. It is this, the pale bark (*Cascarilla fina* of the Spaniards), which is ordered in the Pharmacopœias, and preferred by the *most judicious* British as well as Spanish and American practitioners,—by all, indeed, who form their estimate from extensive experience, unbiassed by the prevailing mania after the *new remedies*, as they are called. By such, however, we find the contrary maintained, both at home and abroad, and that the yellow bark must be best, because it yields the vaunted alkaloid *quinine*, whilst the other affords little or none.

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* I neglected to notice, at the time, what kind of bark the sample was from with which I made these trials, as when I commenced it was not with intention of experiment: I can only say, it possessed all the sensible qualities of the very best bark: (probably the pale). It was from Apothecaries' Hall, Glasgow, and was sent out for Plantation Reliance, Essequibo, in 1826 or 1827.

In France, the quinine is cried up as vastly superior to the analogous alkaloid cinchonine,—an opinion which will, of course, be more or less disseminated here. It must be remembered that the *quinine* was a French discovery, and an extraordinary lucrative one; whilst the other nearly identical element (which may be with more propriety termed a *discovery*) was *previously* made by a British physician, as noticed before.

We observe the following notice of cinchonine in a late French work:—" Ses vertus étant inférieurs, diton, de quatre ou cinq fois à celles de la quinine, on ne s'en sert guère en médicine, non plusque des composés dont elle fait la base."—*Pharm. Univ.* 1828, p. 414.

I shall only observe here, that the well known and established superiority of the pale bark, which gives no quinine, is a poor presumptive proof in support of such pretensions. Here the quinine has been prescribed in larger doses than cinchonine. Mr. Brande remarks, that cinchonine has been less employed in consequence of the comparative cheapness of quinia, an analogous principle, &c.—See "Man. Ph." p. 62, et seq.

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* Whoever would acquire the best information respecting the botanical history of this important genus, should consult especially the *Flora Peruv.*, and that valuable and splendid work, Lambert's *Illustrations of the genus Cinchona.* This munificent botanist has described and figured numerous species of this genus. The herbarium, presenting a still greater number of distinct and well-defined species, of which Mr. L. had the good fortune to become the purchaser, together with those derived from various other sources, form altogether a collection which, in the hands of so liberal a possessor and patron of science, may be regarded as a public treasure of great value, as it is freely opened for the inspection of men of science. Dr. A. T. Thomson has also a noble collection of specimens of this genus, which will not a little enhance the value of his instructive lectures on the important science of medical botany, at the New London University.

Previous to describing the method I propose, it is proper to observe, in respect to the bark in powder, that its sophistications are so great, that we rarely meet with it genuine in the shops. Besides, all barks and vegetable substances keep best entire, or not powdered; and they can then always be ascertained if genuine or not. Regarding the sophistications of Peruvian bark, Dr. Thomson says—"Practitioners ought never to purchase bark in a state of powder, for in this state it is always found more or less adulterated." Dr. Paris (Pharmacologia) mentions, that "in a late inspection of the shops in London, the censors repeatedly met with powdered cinchona, having a harsh metallic taste. A less injurious, but equally fraudulent mixture, is the powder of bark, which has been employed in making the extract."—*London Dispensatory*, p. 247—as also the dregs, it might be added, after making infusions, decoctions, and tinctures.

Therefore bark should always be ordered out in the gross or natural state, as brought from South America, excepting a small quantity for present use, to be sent by a trusty correspondent.

The experiments of Fabroni, moreover, prove, that protracted pulverization and exposure to the air have a similar effect on bark to that of long boiling; that is, in a certain degree to fix its principles or diminish its solubility, and, consequently, its activity. He obtained 14 per cent. of extract from cinchona simply bruised; whilst from that reduced to very fine powder he obtained but half that proportion, or 7 per cent. of extract!

"This (the powder) is the best form of the remedy. If it excite nausea or vomiting, or operate as a cathartic, or occasion costiveness, these inconveniences may, in some degree, be obviated by combining it with aromatics, opium, or a cathartic, as circumstances direct."

"Mutis, conceiving that fermentation is the best method for extracting the active part of cinchona, had proposed to make a beer of it, by fermenting 1 part of the bark in powder, with 8 parts of honey or sugar, and 80 or 100 of water. And Alibert, having persuaded a brewer to make some beer with cinchona, administered it to convalescents weakened by protracted intermittents, with the best effects."—*London Disp.*, p. 247. To the utility and efficacy of this method I can bear testimony.

Here I may observe, that certain eminent writers on Pharmacy have asserted, that the Angostura bark (*Galipea officin.*) does not cure intermittents. See "Edin. Disp.," "Brande's Manual of Pharmacy," &c.—Possibly, in the usual mode of employment, this may be correct; but, from adequate experience, I can assert, that a preparation analogous to that mentioned below, as employed at Angostura, is the most successful remedy both for remittents and intermittents—and those, too, of the most malignant and dangerous kinds, in which the cinchona, alone, is well known to be worse than useless. In such cases, therefore, the former is much to be preferred.

During three years that I sojourned in the Orinoko, I had frequent opportunities of witnessing the superior efficacy of this preparation, not only for convalescents, but in the most obstinate bilious and malignant intermittent and continued fevers which raged in that country in 1816 and 1817. The cinchona, being extremely scarce, we had recourse to the native bark, just alluded to, Kina de Caroni, so called, in the military hospital, of which I had charge, having, in all, nearly 200 patients, besides great numbers in the town of Angostura.

The Cervesa de Kina (bark beer) was prepared by the natives in large jars, placed in the sun, with sugar or molasses, a piece of bread, and yeast at times, to hasten the fermentation. It seemed to me, that the caroni bark thus prepared equalled the cinchona in the cure of common intermittents, and certainly much excelled it in malignant fevers and dropsies, diseases which were surprisingly prevalent and fatal throughout the province in 1817, combining at once the miseries of war, famine, and pestilence. This, perhaps, was also partly owing to the depressing passion of fear and perturbed spirits, the place being then besieged by the patriots. The preceding was the method chiefly adopted with both these barks in Spanish Guiana*.

I at first imagined, that fermentation would deteriorate the powers of the bark; but experience soon removed my prejudices. I found withal, that the bark retained its bitterness, its slight astringency, and peculiar odour in this infusion. It would seem that its active constituents, far from being subverted, are very materially increased in power by this method; and there can be little doubt, that its alkaline and febrifuge elements will be evolved and exalted by means of the solvent power of the acid and alcohol which is formed in the process of fermentation. We have reason also to suppose, that the antiseptic powers of the bark are much increased in this preparation; and this, if true, would especially recommend it in foul ulcerations, and in all putrid diseases. It appears to be a very eligible preparation in hospital practice, or where the bark is constantly wanted in considerable quantity; but may be less convenient in individual cases, as it requires some time for the preparation, and may succeed best in some quantity. Of all preparations of the bark, it is certainly the least unpleasant, being, in all respects, a species of ale or beer.

The addition of the bueyari bush rope (*Aristolochia Guianensis*) augments the efficacy of the bark in a very eminent degree. I have given the bark, mixed with the infusion of this bitter rope, with marked advantage; but, more recently, since the idea occurred to me of the peculiar uses of the vitriolic acid as eliciting the febrifuge principle, I have prescribed it in a compound infusion, after the following formula.

Take of good Peruvian bark, well bruised, or recently powdered ℥j.
Bueyari stem, well bruised, ʒij.
Ginger root, powdered.
Orange or lemon peel, each ʒj.
Liquorice root, powdered, ʒiij.

To these ingredients, add first a small glass of brandy or other proof spirit, with ʒij of dilute sulphuric acid or elixir of vitriol previously mixed; and stir up the mass. Then add half a pint of boiling water, stir it, and keep the vessel closely covered, near the fire, at a scalding heat for an hour or two, according as the ingredients may be more or less reduced or pulverized; as the coarser they are the more time is required for infusion. A pint of boiling water is now to be added; and the digestion continued for an hour or two more, stirring it occasionally, or shaking, without uncovering it. The infusion may afterwards be strained, well pressed out, and bottled, adding two or three bruised cloves, or a drop or two of the oil. —As the pale bark contains chiefly cinchonine, which is said to be most soluble in the muriatic acid, this, if preferred, might be substituted for the sulphuric.

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* The same plan was pursued with the sarsaparilla, with which they used to prepare a beverage of extraordinary efficacy in venereal and rheumatic disorders, for they rarely made use of mercury. It was, indeed, the best kind of sarsa, fresh and genuine from the Rio Negro.

Two or three ounces of good bark, prepared in this way, will go as far, in the cure of intermittents, as two drachms of quinine; and, as a general tonic and restorative remedy, its power is vastly superior to quinine, and may be considered as inferior to no tonic whatever.

The efficacy of the formula is improved by the addition of 4 or 5 grs. of ipecacuanha along with the other ingredients, the beneficial effects of which, on the digestive functions, are much greater than might be expected from so small an addition. This compound is best calculated, both for arresting the progress of intermittents, and for a general tonic in weakness of stomach and loss of appetite, and it is especially useful in remittents, in typhus, and malignant forms of fever. By strengthening the stomach and bowels, it proves the most effective remedy against the breeding of worms in children, *i. e.* after their expulsion by cowhage, or other proper vermifuge; the dose in such cases being a tablespoonful or two, according to the age of the child.

The reason for employing but a small quantity of water at first, is to avoid overdiluting the acid, by which its energy, as a solvent of the quinine or alkaloid, might be rendered less efficient. It is also of much importance first to moisten the ingredients with a little spirit. It serves as a medium to produce a far more perfect combination of the active elements of the compound with a watery menstruum, than could possibly be effected without it.

This method, it is presumed, may be found very useful in similar pharmaceutic operations, as combining to a considerable extent, and at a trifling expense, the advantage of both the spirituous and aqueous menstrua. Since the idea occurred to me, I have tried it on various other substances. It is necessary to witness the effect, in order to believe how much this small quantity of alcohol, *previously added*, will forward the evolution of the more active principles of vegetables. By this method of preparation we are sure of retaining all the active elements of the bark in solution.

The infusion may be taken in the dose of a wine-glass full once in two, three, four, six, to twenty-four hours, according to the intention or urgency of the case in hand *. When the infusion is strained, (as the virtues of the ingredients are not yet exhausted,) another pint of boiling water may be added and digested upon the dregs. This second infusion may be taken with more freedom.

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* I may here notice a fact which has often struck me forcibly, viz. that the vegetable infusions in general, whether febrifuges or general tonics, cathartics, alteratives, &c., are found to be sensibly more efficacious when taken *warm:* there is one direct proof I can adduce of this position in the well known action of infusion of chamomile.

This we find prescribed in a cold infusion as a stomachic. (Brande's "Manual of Pharmacy.") Its emetic property, taken in a tepid state, is well known. I have been told, that the country-people, in certain parts of England, as about Wootton, in Oxfordshire especially, frequently use a strong infusion of chamomile as a cathartic, taking it at bed-time, as hot as it can well be swallowed, and that, in this way, it does not produce emeses, but, as we should naturally infer, acts presently as a potent sudorific; and the following morning as a gentle but effectual purgative. This is a very common resort, as I am assured by a young man acquainted with that part of the country, Mr. E. Charles Prince; and it is there considered as one of the best of remedies in indigestions, colic pains, and obstructions of the bowels, especially when arising from cold, which we know to be the more common exciting cause of those disorders.

To this I may add, that the caroni bark (*Galipea off.*) is employed in a fresh state by the natives on the place of its habitat, precisely in the same manner, and with the same effects, as just noticed of the chamomile; and analogous to this,—a cup of strong coffee, taken hot on an empty stomach, will often produce a similar effect, and that very speedily and pleasantly, '*cito, tuto,* et *jucunde*'; in respect to time, indeed, presenting a singular anomaly with *the tardy* operation of the chamomile.

This, perhaps, is explainable, with respect to vegetables in general, mainly upon the well-known fact, that hot water will take up and hold more of their active principles in solution, than it can do when cold; yet, this is probably not the only reason,—it is partly owing, no doubt, to their more perfectly assimilating and entering the circulation, in this more perfect state of solution; at all events it is certain, that the temperature, in which we exhibit the vegetable infusion, appears to be a matter of more importance than is generally imagined.

We thus have a preparation to be depended on, as containing all the active properties of the bark, even its aromatic parts, in a form almost as pleasant, and as easily swallowed as a glass of beer. That the preparation here offered must contain all the energetic properties of the bark, is fully evident, if the sulphuric acid does really dissolve and develop from the bark an alkaline base, or febrifuge element, as cinchonine and quinine; and of this no doubt can be entertained, as otherwise all that has been said of such elements must be nought but sheer pretence and quackery *.

* This paper is composed from notes, written at various times, but chiefly in the year 1827. Since which, I have employed the compound infusion of cinchona, in numerous cases, with the most decided success, in my own family and that of others, both in Demerara and since our residence in England; and having tried it under many variations of the formula, I give the decided preference to the foregoing prescription.

The infusion of the bark, with a small portion of acid and alcohol, prepared in this way, is found to possess all the flavour and odour of the bark in powder, and I doubt not it contains its entire medicinal properties; and by an operation sufficiently simple,—free from the absurd processes so justly deprecated by Dr. Paris, in the preceding quotations; and not only are the entire virtues of the bark thus evolved, but they are much enhanced when the additional articles are employed, as the bueyari or quassia, ginger and orange peel, as well as the acid. If desired to diminish the bulk, and render the compound more portable, when required for carriage especially, it is only requisite to increase the quantity of spirit, and diminish that of the water—in short, to employ proof spirit or a weaker menstruum, at the option of the practitioner.

It is to be considered, that spirituous menstrua will take up a larger portion of the active elements than an aqueous one can do. Upon the foregoing principles, therefore, we shall be enabled to form a very concentrated tincture of this sort, to the exclusion of some inert vegetable mucilage, which a more watery menstruum must hold in solution. If proof spirit be employed, the menstruum need not exceed the proportion of $\frac{1}{8}$ the quantity of the *infusion* above stated, using all the same ingredients and their quantities as above. It may be digested with a little heat in a bottle lightly corked.

A very small portion of acid is required to neutralize the alkaline principles of the bark. See "Edinburgh Medical and Surgical Journal," vol. xviii., p. 152, and table 155, where it is stated that cinchona combines with sulphuric acid, in the proportion of 100 parts to about 13 of acid, and kina with 11 parts of acid; on an average of the two, about 100 of the base, or alkali, to 12 of acid. It

In this acidulated infusion, we obtain all the elements of the bark, excepting the woody fibre, and perhaps a small portion of resin; whereas, in the process for preparing quinine, the tannin of the bark, which gives it a light styptic or astringent taste, goes past with the liquor or decoction, together with much of the native proper bitter of the bark, as found by experiment, the gummy extractive, resinoid colouring matter, and kinate of lime, while the essential oil, in which the aromatic principle resides, is necessarily dissipated in the

It has been found that the bark, exhausted of its soluble parts in the preparations of extracts, decoctions, &c., still yields nearly an equal portion of the alkali, or quinine. (See "Dr. Johnson's Med. Chir. Journal" for October, 1825.)

Dr. Paris, vol. ii. p. 249, has cited the "Edin. Med. and Surg. Journal," (*vide* next page,) and in some measure seems to have given into the error there noticed. "With respect to their physiological action, it may be stated, that they (the alkaloids) would appear to concentrate in themselves the characteristic properties of the vegetables to which they belong; and yet, although their effects are much greater than those of the undecomposed vegetables, the same quantity of alkali is not so powerful in its pure form, as in its natural state of combination. Thus 1 gr. of morphia produces no more effect than 2 grs. of Turkey opium, which does not contain more than a sixteenth part of the alkali. To explain this loss of efficacy which usually attends our attempts at concentration, the reader is referred to the observations which have been already offered upon this subject in the first volume of this work, page 283."

If, then, 1 gr. of morphia be only equal to 2 of crude opium, and if 16 grs. must be expended to procure the grain of morphia! the concentration certainly does not appear so very striking, nor the effects so much greater than in the undecomposed vegetable. It appears, then, that a sort of *Hibernian* gain alone, is the result of the tedious process for procuring morphia!

A case is related, in a recent number of the "Bull. Univ." of a person having swallowed 24 grs. of acetate of morphia and recovered, although no medical aid was afforded for ten hours, and it was probably superfluous, as its sedative or narcotic effects must then have been *spontaneously* subsiding. This shews how far the vaunted concentration and pretended power is to be depended on: had it been of *crude opium* the case would, doubtless, have ended differently. Such facts deserve to be duly reflected on by the more cool, judicious and philosophic chemist, amidst the popular mania after morphia, quinine, &c.

The discovery of new vegetable elements, now forms an epocha, equal to the recent tide of discovery of new metals and minerals. A most admirable phraseology has been adopted by certain sagacious discoverers. —"Huttenschmid y a decouvert deux substances nouvelles, in Cortex Geoffroyæ probablement de nature alcaline, a aux quelles il a donné les noms de *Jamaicine* et de *Surinamine.*—*Pharm. Univ.* vol. i. p. 606.—All the colonies, islands, &c. will thus furnish a most elegant and appropriate nomenclature for the new principles to be discovered!

boiling. How, then, could it ever have been imagined for a moment, that *a single insulated principle* could possess the exalted virtues arising from a combination of the *various active elements* of Peruvian bark* ?

The results derived from the use of this preparation, in intermittents, in many cases of general debility, &c. give me reason to consider it as the most effectual and certain preparation that can be devised, either as a febrifuge, or general tonic, and I may say corrector and alterative in loss of appetite, weakened state of the stomach and digestive organs, and their concomitant evils, as nervous weakness, spasms, gout, dropsies, ulcerations, scrofula, tubercular consumption, leucophlegmasia, &c. in such cases, it forms a most valuable remedy, taken alternately with the muriated tincture of iron. In those chronic maladies, it would often be of advantage to increase the quantity of the acid.

From the trials I have more lately made with it, I am fully convinced, that this preparation is a far more effectual febrifuge than the same quantity of the best bark taken in substance or powder. It always sits easy on the stomach, and is never disgusting, especially if at the end of the operation, you agitate with the infusion three or four drops of the essential oil of cinnamon, or of cassia, dissolved in a little brandy.

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* Such reflections could not have been adverted to by the learned editor of the "Edin. Med. and Surg. Journal," when he expressed himself at a loss to account for the inferiority of these alkalies to the entire or undecomposed vegetables. "In employing the alkalies instead of the vegetables, not only is the expense augmented by the process for their preparation, but they actually do not possess nearly the same degree of energy as they do in their natural state of combination; that is, if an ounce of any vegetable matter contains 30 grs. of an alkali, those 30 grs. are not so powerful in their pure state as when they are united with other principles, to form an ounce of the undecomposed vegetable." To these judicious remarks, he strangely adds, "We have no means of conjecturing whence this proceeds." Vol. xviii. p. 162; and at page 155, it is remarked, that "These vegetable alkalies are endowed with a powerful influence upon the animal economy, and it is a law, hitherto universal, that they concentrate in themselves all the physiological properties of the vegetables, to which they belong! It is worthy of remark, however, that although their effect is much greater than that of the undecomposed vegetables, the same quantity of alkali is not so powerful in its pure form as in its natural state of combination." Nothing could be more just than the last sentence, whilst it is a direct contradiction to the preceding one. It is curious to observe the conflicting opinions which arise from false premises: or does this originate in an undue deference to the *Magni Nominis Umbra*—an overweening confidence in annunciations emanating from men of high celebrity?

One ounce of good bark, prepared in the foregoing manner, will generally be found sufficient to remove an intermittent, especially if taken warm, and freely, just before the expected return of the paroxysm. The cost of the articles will be a mere trifle. In want of the bitter rope, the lignum quassiæ will be found a good substitute, and may be used in the same proportion *.

In conclusion, I must apologize for the length and imperfections of this paper; I can only hope it will serve as an inducement to abler minds, to investigate the important subject of which it treats.

J. H.

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* Huxham's celebrated prescription, which still holds its credit in medical practice, was a vinous tincture of the bark with orange-peel, snake-root, saffron, and cochineal, which he directed to be given in wine and water with addition of elixir of vitriol, and which, he observed, he had long used with success in intermittent and slow nervous fevers, as well as in the putrid, pestilential, and petechial. He says, that " although very profuse sweats may be hurtful in those and other fevers, yet gentle and moderate sweats are ever to be encouraged by diluents. And, as these fevers are frequently long protracted, and thus exhaust the patient, it is requisite to support the strength with liquid nourishment and generous old wine, acidulated with lemon juice, and impregnated with aromatics, as cinnamon, orange-rind, red roses, and a few drops of elixir of vitriol, &c." He adds, that " the Asiatics and other nations, among whom pestilential disorders are much more rife than with us, lay more stress on the juice of lemons than on the most celebrated alexipharmic." (See "Huxham on Fevers," p. 122.)—This remark is verified with respect to the popular treatment of these disorders amongst the French and Spaniards in the tropical parts of America; and this practice, together with the use of baths and frictions, is attended with much greater success, than that which results from the common European practice with calomel, antimonials, strong purgatives, &c.

The compound tincture, with elixir of vitriol, Huxham called an excellent *antiseptic alexipharmic*, which it really is; but there can be no doubt its efficacy would be much augmented by *previously* adding the acid.

Though the foregoing remarks may be thought not entirely relevant here, yet it is hoped that the experience of Dr. Huxham will be a sufficient apology for the brief mention of his opinions.

Some Notices of the great Storm and Flood which occurred in the Counties of Aberdeen, Banff, Elgin, and Nairn, and parts of Mearns, Angus, Perth, and Inverness, on the 3d and 4th August, 1829.

[In a Letter from the Rev. JAMES FARQUHARSON, of Alford, Aberdeenshire, communicated by Captain SABINE.]

AT the period of this storm I was residing temporarily at the village of Ballatu, situated on the river Dee, about forty miles westward from Aberdeen; and the house in which I lodged is the nearest to the river and bridge which was swept away, having from the windows a full view of both within forty yards.

On the day preceding the storm, the weather was mild and warm, and the sky, shrouded by dense strata of clouds moving slowly eastward, giving indications rather of the approach of a westerly gale than of the terrible catastrophe which followed.

The morning of the 3d August was ushered in with a drizzling rain, with light wind from a point east of north. About 9 A. M. the river began to be slightly discoloured with flood-water, indicating more rain in the mountains than we yet had in the valley. About mid-day the rain intermitted for two hours, but came on again before 2 P. M., and the wind shifted to a point west of north, the river slowly and uniformly rising all the time; but up to 5 P. M. there was nothing unusual either in the force of the wind or quantity of the rain.

From about that hour the elements suddenly assumed all the characters marked in the common descriptions which we have received of the tropical hurricanes. The wind blew with extreme violence in sudden gusts, and whirlwinds, whose centres were well defined to the eye, as they crossed and vehemently agitated the surface of the river. It is, probably, similar whirlwinds which are described to us, when in the accounts of the tropical hurricanes we are informed, "that the wind blows from all points of the compass at once." The centres of those in question all moved forward in parallel paths from a point west of north; and so great was their force, that having occasion to give orders about my horse at a place about 150 yards from my lodgings, I had several narrow escapes from being violently thrown down, or dashed against the neighbouring houses.

A very heavy rain now fell without intermission; and at half-past five, lightning and thunder began, and continued, with short intervals between the flashes, till 8 P. M., when it ceased. The lightning, with the exception of the two or three first flashes, which were similar to the ordinary forked lightning of this climate, followed by acute reports, was of a character which I have seldom witnessed. It presented the appearance of extensive blazes or sheets of flame dispersed over a large space of the heavens; and, although it was often very near, was followed by no acute report, but by a grave rumbling noise like the rolling of distant drums, passing often into a hissing sound like that of sky-rockets.

There was no sensible intermission of the rain till about 9 P. M.; after which it consisted of a succession of showers, very gradually diminishing in heaviness, and coming on after gradually increasing intervals of time, till 6 A. M. of the 4th, when it ceased entirely. The wind abated with the rain.

The river rose gradually but steadily, after 6 P. M. of the 3d, till 11 P. M., when it was about 8 feet above its usual level. From that hour, till 12 P. M., it rather fell; but towards 1 A. M. of the 4th, the flood-water of the head river in Braemar, which drains an extensive valley among the highest Alps of Scotland, by a number of branches uniting like the radii of a circle, and passes through a narrow defile at Invercauld, about 20 miles higher up, having at length reached Ballatu, the river again rose with great rapidity. For a short time between 1 and 2 A. M. the rise was at the rate of one foot in ten minutes, as I had an opportunity of ascertaining in the lower story of the house where I lodged, which was by that time inundated; and the ultimate rise of the river, which occurred about 5 A. M. was at this place about 13 feet above its ordinary level. The greater part of the inhabitants of the village, which is about 11 feet above the river, were driven out of their houses, which are mostly of only one story, at 2 A. M.

The bridge at this place, built of granite about twenty years ago, consisted of five arches, having a water-way, in whole, of 260 feet. As the bed of the river consists of rolled pieces of granite, and gneiss of unknown depth, the piers of the bridge had been laid on platforms of wood, let down about 5 feet below the bed, and this expedient had been sufficient for bearing the vertical pressure, and up to the time of this flood there was no settling or flaw in the masonry. It became apparent, however, before dark on the 3d, that there was not sufficient water-way for this unusual flood; and a damming back of the water took place at the bridge, which caused a sudden fall of about 4 feet perpendicular into the openings of the arches. The height of this fall had, no doubt, increased with the rise of the river through the night, but could not be seen for the darkness. The effect was, that the rolled materials of the bottom were swept away, and the piers were at last undermined. The upper half of the north pier fell up the river about 3 A. M. of the 4th, letting down parts of the conterminous arches. The bridge remained in this state about an hour and a half, when the south pier fell wholly up the river, letting down the two south arches; and about ten minutes afterwards, the centre arch and remaining portion of the two north arches fell, apparently, from the spreading of the piers by the lateral pressure of the arches. The materials of this great mass of masonry have, in a great measure, disappeared in the bed of the river, being, no doubt, buried in the deep pit, scooped out under it before it fell.

The ordinary depth of the river at the shallows near the bridge, is from 2 to 3 feet, and the fall, in its course, about 20 feet in a mile.

This great storm and flood extended, it seems, nearly simultaneously, and in equal violence, over a space of about 5000 square miles;—being that part of Scotland cut off to the north-eastward, by two lines, from the head of Lochrannoch, one towards Inverness, and another towards Stonehaven*. All the rivers within that space were flooded, proportionably, with the Dee. The damage suffered, in consequence, in the destruction of bridges and roads, and lands, buildings, and crops, along the courses of the streams, has been very great, and has been estimated at half a million sterling. Happily few lives were lost.

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* The storm extended much farther, but without the great rain.

The effects of the storm on the grain crops, excepting where they were overflowed by the streams, were less injurious than its severity might have led one to anticipate. They had not yet passed the full vigour of vegetation, the ears being, in few cases, fully shot out; and a remarkably fine day with much sun, on the 6th of August, enabled the plants to rise up again; and, ultimately, a harvest, not very deficient, was reaped in all the more productive districts.

Being from home, I had no opportunity of observing, myself, the state of the barometer or thermometer at the period of this flood; but have learnt since, from unquestionable authority, that nothing remarkable was indicated by either. At the commencement of the storm, the barometer was a little above its mean height; and its fall, during the progress of the storm, was trifling. The thermometer was nearly at temperate all the time. I have learnt from Mr. Murdock, at Huntly-Lodge, who registers a rain guage, that the fall between 5 A. M. on the 3d, and 5 A. M. on the 4th, was, at that place, 3¾ inches. The larger proportion of this fell between 5 and 9 P. M. on the 3d.

The effect of running water, in moving stones and heavy materials, has been, in many instances, curiously illustrated by this flood. The River Don has, upon my own premises, forced a mass of 400 or 500 tons of stones, many of them 200 or 300 pounds weight, up an inclined plane, rising 6 feet in 8 or 10 yards; and left them in a rectangular heap, about 3 feet deep on a flat ground; and, singularly enough, the heap ends abruptly at its lower extremity. A large stone, of 3 or 4 tons, which I have known for many years in a deep pool of the river, has been moved about 100 yards from its place; and an immense collection of small stones, of many hundred tons, gathered from the arable land, and laid down as an embankment at a turn of the small river Leochell, has been carried down, and left on the field below, not promiscuously dispersed, but as if artificially collected into large and deep heaps.

Observations on the Teeth of the Erinaceus Europæus, Urchin, or common Hedgehog.

To the Editor of the Quarterly Journal of Science.

Sir,

Already, more than once, I have had occasion to notice the uncertainty, as primary distinctions, of those popularly favourite zoographical characters, the teeth, which are found, not only to associate animals essentially dissimilar, and to separate others as intimately allied, but also to be so ambiguous in themselves, that the same individual has been placed in different groups, not only by different persons, but even by the same zoologist at different times: the Manupeda and Alipeda have afforded us examples, and the other types will give their testimony also. But, perhaps, as a solitary proof of this perplexing ambiguity, there is not a more striking or notorious instance to be met with, than in the very discordant accounts which several most distinguished naturalists have given of the dental system of the Erinaceus Europæus, or common hedgehog; and which, as it is included in the following outline of the arrangement of the Quadrupeda, demands, in the first place, our attentive consideration.

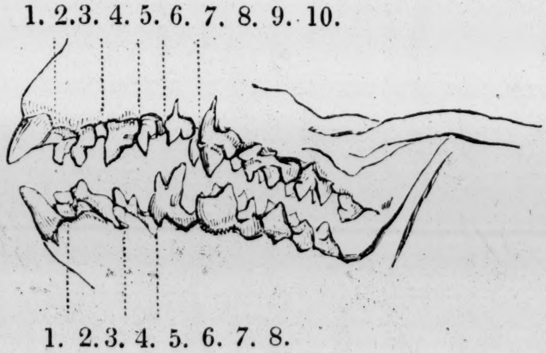
Linnæus, Brisson, Pennant, Shaw, Blumenbach, George Cuvier, Illiger, Desmarest, and most zoologists, describe the hedgehog as possessing, and, indeed, characterized by the possession of canine or laniar teeth, though they vary in their statements as to the especial number: while the same Pennant, Frederick Cuvier, and Latreille deny the existence of any canines at all. On this subject, Pennant writes, "it is impossible to allow the harmless hedgehog to be the companion of lions, wolves, and bears;" yet such is the common arrangement, and such the arrangement that he afterwards adopted. G. Cuvier says, "les Hérissons, &c., *ont le canines plus courtes* que les antres dents;" and Illiger describes them as ambiguous; instead of being, as Shaw declares, "*perfectly* those of the order Feræ." The following tabular conspectus will shew the difficulty of discrimination from the extraordinary discrepancies which exist among the most astute zoologists: it will also prove that the same teeth have been, by different authors, called, at one time, incisors, and at another canines; that the canines, in like manner, have been occasionally denominated molars; and again, that even to the same teeth all the three several epithets have been indifferently ascribed.

Linnæus	incisors $\frac{2}{2}$	canines $\frac{2-2}{2-2}$	recumbent.
Pennant	$\frac{2}{2}$	$\frac{0-0}{0-0}$ or $\frac{4-4}{3-3}$	molars $\frac{5-5}{4-4}$
Shaw	$\frac{2}{2}$	$\frac{5-5}{3-3}$	$\frac{4-4}{4-4}$
Blumenbach	$\frac{6}{6}$	$\frac{3-3}{1-1}$	$\frac{4-4}{4-4}$
Illiger	$\frac{6}{6}$	$\frac{2-2}{1-1}$	$\frac{5-5}{4-4}$
George Cuvier	$\frac{6}{6}$	$\frac{2-2}{1-1}$	$\frac{4-4}{4-4}$
Frederick Cuvier	$\frac{6}{2}$	$\frac{0-0}{0-0}$	$\frac{7-7}{7-7}$
Griffiths' Cuvier and Desmarest	$\frac{6}{6}$	$\frac{1-1}{1-1}$	$\frac{5-5}{4-4}$
Specimen	$\frac{6}{6}$ or $\frac{6}{8}$	$\frac{3-3}{1-1}$ or $\frac{2\,.\,2}{0\,.\,0}$	$\frac{4-4}{4-4}$ or $\frac{5-5}{4-4}$

Had these contradictory accounts been published of some rare animal, the paucity of examples submitted to each inquirer, and the casualties to which all organized beings are liable, might have explained the variations, or, at least, have proffered an excuse for the inconstancy of the accounts; which now must be confessed to be owing to the questionable characters of the teeth themselves. Still, as the doubt has directed the attention of naturalists especially to this point, it is curious that the same objects should have been so variously described, that the same premises should have led to such different conclusions.

The dental developement of the hedgehog, when critically and scrupulously examined, is perceived to adumbrate with one system of teeth, the type to be perfected in another; and this so closely, that perhaps it might, by the captious, still be questioned to which group we should refer them. Superficial observation might be led by the spinous integument to connect them with certain rats, or rather with the porcupine, to which they are undoubtedly, in some respects, allied; but their habits of life and food, certainly, associate them much more closely with the subterranean insectivorous Feræ; and they are, like them, beasts of prey, as far as their strength and circumstances permit. In tracing the dental degradation alluded to, it must be borne in mind, that the front teeth, which, in the Feræ, and many other beasts, from their conical shape, little fitting them for cutting, have been called dentes primores, instead of dentes incisores, and which are usually shorter than the canines or laniar teeth; in the majority of cases are set side by side, forming a line in front of the mouth; in the hedgehog have their position reversed: two in each jaw being advanced in front, and greatly elongated, while four others follow; two, at least, on each side the mouth in either jaw, and almost recumbent. The canines are certainly ambiguous both in shape and number, being shorter than the long incisors, and having, in the under jaw, somewhat the form of bicuspidor false molar teeth; and, by Frederick Cuvier, they have so been named. From their position, however, and their single fangs, together with the predaceous habits of the animal, they are more properly considered as transitional canines; indeed, two in the upper jaw decidedly retain the laniar form, while, in the lower jaw, there are, as certainly, no true canines at all.

1. The Rodent Incisor of Upper Jaw.
2. 3. Degenerate Incisors.
4. True Canine.
5. Transitional, or
6. Degenerate ditto.
7. 8. 9. 10. Molars.



1. Rodent Incisor of Lower Jaw.
2. 3. Transitional Recumbent do.
4. Ambiguous Tooth in the place of the Canine, accompanying the inciscrs, and having a Molar form.
5. 6. 7. 8. True Molars.

Thus it becomes manifest, that the teeth described by Linnæus as canines, two on each side recumbent, are, in truth, degenerate incisors; and the anterior of his molar teeth are the real transitional canines. Shaw's canines included the recumbent incisors, together with the canines and anterior molars of Pennant and Linnæus. Blumenbach distinguishes the incisors correctly, but his canines include the anterior molars of Illiger and the elder Cuvier; while Frederick Cuvier includes among his molars, not only the canines of the other writers, but even the two hindmost incisors on either side of the lower jaw.

Hence the hedgehog forms an admirable illustration of the transitional structure, which connects two tribes of quadrupeds, whose habits and dispositions are essentially distinct, the Rodentia and the Feræ; the one a race of harmless, almost defenceless, herbivorous beasts; the other a series in which power and ferocity are developed to their full extent: and yet so gradual is the change from these to those, from the lion, the tiger, and the sanguinary wolf, to the mouse, the squirrel, and the timid hare,—from those which have the tearing teeth large, strong, and prominent, to these in which they are obsolete, obscure, or absent, and even to such as have no teeth at all; that in the osculant genera, as the shrew, the hedgehog, and the mole, their presence is alternately admitted and denied; at one time associating them with the gnawing beasts, at another with the beasts of prey.

This circumstance, which hath, as might have been supposed, caused much perplexity to such zoographers who would seek to *divide*, as well as to *distinguish*, the several provinces of nature, is one of those instances which go far to prove that, in our systems, we should strive to indicate the connexions, not to isolate the several parts; to unravel the associations, not rudely to sever those intermediate bands which harmonize dissimilar, and otherwise discordant groups; for then the very obscurity which, by the mere systematist, is esteemed a stumbling-block, to the physiologist becomes a pharos, and is regarded not only as an interesting, but an important and valuable guide.

I have the honour to remain,
Yours obediently,
GILBERT THOMAS BURNETT.

Theatre of Anatomy, Great
Windmill-Street.
November 30, 1829.

Illustrations of the Quadrupeda, or Quadrupeds, being the arrangement of the true four-footed Beasts indicated in outline.

Tetrapoda, Quadrupeda, the originals of our naturalized Tetrapods and Quadrupeds, words formerly synonymous, and equally translatable four-footed, have, however, in the progress of science, become appropriated to different meanings; and this is one of the advantages which our language gains by engrafting into it classic terms; for shades of difference are thus succinctly and elegantly expressed without a periphrase; an important power, of which dialects, less mixed, are utterly unconscious and devoid.

The earlier naturalists, to whom little more than the external structure of animals was known, and by whom dissections were but seldom practised, consequently selected for their zoographic signs the most obvious features of exterior form, such as the wings, the feet, the claws, the teeth, &c., aided, when observation failed them not, by the habits of the several grades. Hence the Tetrapoda of the Greeks, and the Quadrupeda of the Latins, were terms literally and indiscriminately applied, and included the four-footed reptiles, along with the four-footed beasts. The frog, the toad, the tortoise and the crocodile were associated, in one compartment, with the lion, the deer, the elephant and the mouse; the two series being interdistinguished as the oviparous and the viviparous sections. Yet, as further investigations shewed, not only that these were more or less clothed with hair, while those were invested with scales, or naked; but also that the one had warm blood and a double heart, the others a single heart, and varied in their temperature with the medium around them; that the former were more nearly allied to serpents, and the latter to whales, than either to each other: the number and developement of the feet proving fallacious, as a primary distinction, was very properly eschewed; and other less defective means proposed and followed in their stead.

The discovery of a proper circulation, which threw so much light on the animal economy, and solved so many problems in physiology, had its influence on physiography likewise; and the Linnæan method, formed chiefly on this principle, in great measure superseded the respiratory classification, recommended by Aristotle, and adopted by Ray and others. But the nervous system is the idol of the present day, and to this the circulating seems doomed to yield as the respiratory succumbed to it; for, according to the developement and concentration of the brain and nerves, a primary distribution has been effected by the celebrated Cuvier, who divides the whole animal reign into the vertebrate and invertebrate series; the latter of which is again subdivided, before the distribution of either into classes. Of the successive prevalence of these several schemes, founded on the external or internal structure, on the respiratory, the circulating and the sensorial systems, more shall be said in a future essay; the mere recollection of their epochal predominance is all that will now be useful: each plan has advantages and disadvantages peculiar to itself; all assist in part to explicate the oracles of nature; and it is only when either becomes advanced unduly, that those sections are rendered needlessly obscure, that its compeers are probably fitted to elucidate in turn. Therefore it is an object worthy the attempt, so to reconcile the ancient emphatic terms deduced from external and obvious characters, with the internal and essential structure developed in modern times, that while we retain, as far as possible, the familiar and expressive nomenclature established by the one, the definitions may be corrected to include the important anatomical discoveries of the other. Thus, for example, while all the warm-blooded, hairy, unfeathered brutes are known as beasts, and the cold-blooded, lung-breathing animals are classed as reptiles, the number and form of their limbs, and the popular epithets derived therefrom, although arbitrary and erring as primary, are most convenient and just as subordinate, distinctions; and the essential peculiarities of the intimate structure and constitution of the separate grades is well predicated by calling the four-footed beasts Quadrupeds, the four-footed reptiles Tetrapods; thus making a single expressive word include, not only the obvious external form, but also indicate the physiological peculiarities of their internal structure.

Great as are the varieties of external form, and numerous as are the modifications of internal structure, still there is a relative developement of each; and the outward manifestation of the one is, in general, a sign of the inward developement of the other. The resources of nature are, it is true, so mighty, and our knowledge of her scheme, at best, so limited, that this index is not unfrequently mistaken, and on some occasions has been imprudently propounded as *a necessary condition of existence.* Even the illustrious Cuvier would seem to have been too much dazzled with the brilliant theory of this mutual interdependence of the various internal and external organs; not sufficiently calculating our ignorance of the compensating means by which, as Flemming observes, a state of mutual dependence and reciprocity is maintained, in the midst of a diversity of combinations which display, in the most astonishing manner, the endless resources of the wisdom and power of the Great Creator, "whose ways are not as our ways, neither are our thoughts his thoughts." This frequent harmony between the various organs that act upon each other, and this reciprocity which we can often trace between the inner structure and the outer form, has been admirably illustrated by the first-named writer; who observes, "an animal which can only digest flesh, must, to preserve its species, have the power of discovering its prey, of pursuing, of seizing, of overcoming and of tearing it to pieces. It is necessary, then, that this animal should have a penetrating eye, a quick smell, a swift motion, address and strength in the talons and the jaws. Agreeably to this necessity, a sharp tooth fitted for cutting, is never coexistent in the same species with a foot covered with horn, which can only support the animal, but with which it cannot grasp anything; hence the law by which all hoofed animals are herbivorous; and also those still more detailed laws, which are but corollaries of the first, viz. that hoofs indicate dentes molares with flat crowns, a very long alimentary canal, a capacious or multiplied stomach, and several other relations of the same kind." Not to dilate on the many, perhaps, only apparent exceptions to this law, which, in the present imperfect state of our knowledge, certainly do exist, enough has been already done to shew that these reciprocal indications abound; and as far as may be, they should always be sought after: still they are not always to be traced, and when they cannot, although an important point of internal structure must never be neglected, more especially in physiological discussions, still an obvious external character will be commonly the most useful sign; not only in a popular, but also in every system of zoographical arrangement.

This reciprocal adaptation of external characters to indicate internal forms, and this reconcilement of ancient, established and familiar terms, with modern discoveries and attainments, are objects too little and too seldom regarded; and names both perspicuous, and expressive, are too often allowed to become obsolete, or stigmatised as vulgar and incorrect, while others, less energetic and equally objectionable, are as often introduced. Of this, the words ***Brutes***, ***Beasts***, *Quadrupeds*, *Cattle*, &c. &c. now attempted to be re-established, may serve as apposite examples. Thus the word Quadruped, as above defined, when limited to the truly four-footed individuals of the one class, while Tetrapod is restrained to those of the other, may again become a useful philosophic term, after being all but obsolete in science; though in popular language, from its obvious signification, it has always maintained its ground.

Having, on principles similar to these, already applied the word Brutes, to denote mere animals, thus excluding the mental or human grade, and distinguishing rational and immortal man from the mere sensual brutes that perish, (*vide* "Journal of Science," No. vi.) and having defined the first class of brutes to be those vertebrated, lung-breathing, warm-blooded animals, whose bodies are invested with hair, or naked; at least not covered by feathers; which brute animals we termed Beasts, to distinguish them from Birds and Reptiles, and having (in "Journal of Science," Nos. viii. and x.) followed out the distribution of the two first types of this class, which have respectively feet in the form of hands, and feet in the form of wings, the next series will include all the true four-footed mammalious beasts, which will be treated of under the name of Quadrupeds, thus completing, with the Alipeds and Manupeds, the wing-footed and the hand-footed types, the first order of this first class, all of which are characterised by having four well-developed limbs, either in the form of hands, or wings, or feet, possessing breasts and being viviparous; in these points contrasting, as will be more particularly hereafter shewn, with the two other orders of this class; the one of which, as the Ornithorhyncus, although four-footed, is not mammiferous; and the other, as the whales, although mammalious, are not four-footed. The subordinate distribution of the Quadrupeds, or truly four-footed beasts, will depend upon the respective developement of the toes and their relative armature by claws or hoofs; the presence of Marsupia, or the conditions thereof; and subsequently on the characters afforded by the teeth, which, although producing some anomalous associations, when resorted to as a first diagnosis, become most valuable distinctions when used to indicate especial groups.

The quadrupeds, which, as a type, are both mammalious and four-footed, become naturally interdistinguished among themselves by one district, being digitate and having claws: another shewing the toes far less developed, the digits being confined and encased in hoofs: and connecting these two extremes, the hoofed and the claw-footed beasts, there is found an intermediate district, comprising a most curious race of animals, which are naturally, as it were, parturient by abortion, and for the most part furnished with abdominal teat-pouches, resembling, in some respects, an external matrix or living nest; and which præcocinate animals approach, by some of their species, the clawed and digitated, by others, the hoofed extremes of their common type. These three districts are by name the Unguipedatæ (Unguipedates), or claw-footed beasts; the Cornipedatæ (Cornipedates), hoofed or hoof-footed beasts; and the Præcocinatæ (Præcocinates), Marsupiatæ (Marsupiates), pouched beasts, or Mastothecatæ (Mastothecates), teat-pouched quadrupeds; the abdominal sac, by which they are characterized, having been named Mastotheca, or Marsupium. Other names, or modifications of the foregoing, may, and, indeed, have been given to these several districts, still in general they are little more than trifling variations, and scarcely require enumeration even as synonymes. The Unguipedatæ might be called Unguiculatæ, Ungulatæ, or Digitipeda; the Cornipedatæ, Ungulosæ, or Ungulogradæ; the Præcocinatæ, Marsupiatæ, Eplacentaria, Marsupialia, &c. &c.; but the former terms and terminations are, perhaps, the best; especially as regards the last-named animals, which are all præcocinate, although they are not all possessed of a Marsupium.

The quadrupeds being the largest type of this first order of the class of beasts, admit of more numerous subdivisions than either the hand, or the wing-footed *types;* in each of which but one *district* was included, comprising the *races* and the *kinds;* in this, however, three *districts* are found, each containing three several *races,* and each race its appropriate kinds. These districts have been already named the Cornipedates, the Præcocinates or Marsupiates, and the Unguipedates, and of each of them in turn. The indications, however, shall be strictly restrained to outline. (*Vide* Table, p. 348.)

Among the digitated quadrupeds, associated either by having distinctly developed toes, or their digits, if less exserted, being armed with nails or claws, never having hoofs, or Mastothecæ, considerable differences in habit and in other natural characters prevail. One extensive race includes the more or less savage beasts, such as are usually referred to as beasts of prey; and the pugnacious and sarcophagal propensities pervade the entire race, though subject to many modifications, according to the corporeal strength of the several kinds, or the circumstances under which certain individuals may be placed. They are strongest in the lion, the tiger, the wolf, the fox, the weazel, and the ferret: weaker in the bear, the badger, and the racoon; and evanishing in the mole, the shrew, and the hedgehog; by which last the Feræ are connected with another type. In the true Feræ and their allies three sorts of teeth are found, incisors, canines, and molars,—at least there is no decided absence of either; for even when the laniar character is obscure, the transitional canines occupy that space which is left vacant in the Rodentia, or Glires. They have no horizontal motion of the jaw, their smell is great, and their intestines short. They are more or less exclusively carnivorous by nature, according to the relative developement of their normal structure, especially of the laniar teeth; from the lion and the tiger, which have them large and long, to the hedgehog, in which they are ambiguous. This last genus, as hath been already noticed, forms the connecting link between the Feræ and the Glires, as the Hyrax will connect the Unguipedates by the Glires, to the Cornipedates by the Belluæ, as shall be seen hereafter. According to these circumstances, and the habits thereon dependent, the Feræ have been conveniently subsorted into three sections, the Sanguinariæ, the Predaceæ, and the Subterraneæ: the former two have also been termed Carnivoræ, to distinguish them from the latter, which are Insectivorous; and the Sanguinariæ have also been called Digitigrades, from their mode of walking, while the Predaceæ and Insectivoræ are Plantigrades. Indeed, by taking different points of structure and habit as criteria, almost as many distributions might be effected as there are organs in the animal frame; but such would be worse than useless labour; the only legitimate object being to contrast the advantages of the several plans, and select as characters those which are the most obvious, constant, and decisive; not alone because they are common, nor yet because they are original, but because they promise to be the most perspicuous and the most useful. Therefore all those unguipedate pouchless quadrupeds which have three sorts of teeth, whether more or less developed, and whatever be their number, are associated in one race: the Tridentulæ, Plenidentatæ, or Plenidentia of the dental scheme: the savage beasts of the vulgar; the FERÆ of Zoologists. As now enumerated, they are analogous to the Feræ of Linnæus, excluding Phoca and Didelphis; and to the Carnivora of Cuvier, excluding the Amphibia. (*Vide* Table, p. 349.)

The most distinguishing characteristic of the GLIRES, or Rodentia, the second race of unguipedate quadrupeds, is the universal absence of laniar teeth, instead of which is found the signal diastemma, unmarked by even transitional canines; the incisors, usually two in each jaw, are chisel-shaped, and so far protruded as to give these animals a peculiar facility in gnawing, whence one name; their molars are more or less complex and lamellate; in the frugivorous species the crowns are nearly flat; in others, whose food is of a more varied kind, they are tuberculated; in most the intestines are long and the cæcum voluminous. Their feet are little different from the Feræ, but their toes are rather armed with nails, than claws, which are unable to seize and unfitted to tear any living prey; the two bones of the forearm are often united, and can scarcely ever be regarded as having the power of turning, while the Feræ have a limited privilege both of pronation and supination; though considerably less than the monkey type. In like manner, as some of the Insectivorous Feræ have clavicles and others none, so is it with the Glires; and from this circumstance two or even three sections have been distinguished in this race, the first with clavicles, as the beaver, the rat, the squirrel, &c.; the others, with rudiments only, or without, as the porcupine, the hare, and the cavy. The synonymes, as analogous to the several titles of the preceding race, may be enumerated Bidentulæ, from their two sorts of teeth; GLIRES (Liberæ), or free beasts, from their habits; and as now enumerated, they vary little from the Glires of Linnæus, the Rodentia of Blumenbach, the Rongeurs of Cuvier, the Prensiculantia of Illiger, the Celerigrades of Blainville, &c. &c. (*Vide* Table, p. 350.)

The Unguipedates hitherto unmentioned are chiefly characterized by negative qualities; in them even the digits become less and less in number, are less distinct and less exserted; the teeth are never of more than two kinds, usually canines and molars, the incisors are almost always absent; in some, as the Tatusia, and Orycteropus, there are molars only, and in others, even the fibrous molars are wanting, and there are no teeth at all. These animals not only connect the quadrupedous mammalia with the Monotremes, but also by some of their species, as the scaly ant-eaters, to the Chelonian reptiles. From their common want of incisor teeth, they have been called Exincisoria; from the absence of the canines, as well as from the imperfect structure of the molars when present, and the frequent absence of all, Edentulæ, Edentatæ, or Edentia; but, perhaps, from their habits their best name would be SUBFERINÆ, or Semiferæ, wild beasts; thus contrasting them with the Feræ and the Liberæ, the free and savage beasts. As now enumerated, they correspond to the Bruta of Linnæus, excluding the elephant and the walrus; the Effodientia and Tardigrada of Illiger, excluding Prochilus; and the Edentes of Cuvier, excluding the Monotremes. (*Vide* Table, p. 351.)

The PRÆCOCINATÆ (Præcocinates); Marsupiatæ (Marsupiates); Mastothecatæ (Mastothecates); sacculated or teat-pouched beasts, which form the second district of the type of Quadrupeds, strongly characterized by the extraordinary perversion of their genitals, were gathered together from the several orders into which their varied dental developements would disperse them, and associated most philosophically by Cuvier as a separate tribe; and although, in his arrangement, they form but a sub-section of the Carnassiers, this anomaly has been redressed by succeeding writers; for the Carnassiers are defined as possessing three sorts of teeth, while some of the Marsupials want canines in one, and some in both their jaws; being, in fact, sacculated glires: they constitute by this, their index, a series essentially distinct.

But few of these animals were known to Linnæus, and all that were then discovered he included in the single genus Didelphis, which was arranged among his Feræ; and both Blumenbach and Illiger adopted a nearly similar distribution. Did their number require a subordinate demarcation of the district into races, the variety of their dental developement would seem to furnish a convenient guide. Whether future discoveries may suggest more natural groups can only be matter of conjecture.

On the one hand, we find a race of these sacculated animals very rapacious, and possessed of three sorts of teeth, having canines in both their jaws; hence this race may be called the Feridentia: and, were it not for their pouches, they might be arranged with the Feræ, allying themselves to the Insectivora, being chiefly Entomophagous. In another race the upper canines are wanting; these are the Kangaroo Rats: they are chiefly carpophagous; and, from their dental structure, form a connexion between the Feræ and the Glires, as also between one section of the Marsupiates and the other: they may be called the Semidentatæ or Semidentia, and lead to the third Marsupial race, which have no canines at all in either jaw; thus associating with the Glires, and hence termed the Gliridentia. The Pedimanous species of this district would associate the Quadrupeds with the Manupeds; the Balantia shews an affinity to the Pleuropterus of the Alipeds; and the united digits of some other species would mark their affinity to the district which is next to follow. (*Vide* Table, p. 351.)

The CORNIPEDATÆ (Cornipedates), hoofed or hoof-footed quadrupeds, the last district of the present type, are characterized by having all their four extremities freely exserted by their feet terminating in toes more or less numerous and more or less developed; encased in hoofs, which are differently divided: none of them have abdominal pouches, and their dental system is very various. The three races are distinguished by having one, two, or more toes predominantly developed, and inclosed respectively in whole, cloven, and subsulcate hoofs. The elephant, the hippopotamus, the rhinoceros, the hog, and their allies, will form the first race of the hoof-footed district: they are non-ruminant animals, with more than two toes inclosed in hoofs. Some difficulty would seem to have arisen as to the arrangement and the naming of this extraordinary race of quadrupeds. The elephant was placed among the Bruta of Linnæus, and associated with the walrus, the sloth, and the ant-eaters; his Belluæ contained the hippopotamus, rhinoceros, hog, and horse. The association was much improved by Blumenbach, who excluded the Trichecus and the horse, and called the group Multungula. Cuvier, who retained the horse in this section, and added the Hyrax to the assemblage, called the whole, from the thickness of the skin in most, Pachydermata; but the Daman at any rate is not a Pachydermatous animal; and, as we exclude the horse, the name Belluæ, which, in the classic writers, is more especially applied to uncouth, unwieldy beasts, seems the most fitting and expressive term. Had a word been to be invented, as the herds of swine and hordes of elephants are separated from the horse and the ruminating cattle, *i. e.* from the flocks, *Egreges* (out-herds) might not have been an inappropriate appellation for these Belluine beasts; its true meaning would point out their segregation from the flocks and herds; and its acquired signification would obliquely indicate their vast, unsightly forms. They might likewise be called Armenta or hordes; and, from their hoofs, Subungulosa, Multungula, Gravidada, &c. (*Vide* Table, p. 352.)

The Ruminating Quadrupeds, or second race of hoof-footed beasts, form so natural an assemblage, that all writers agree in their alliance, however they may differ in the names that are severally attached thereto: little comment will, therefore, here be needed. PECORA, Pecudines, or Cattle, would seem the most familiar and the best appellations: from their cloven hoofs they have been called Bisulca, Binungula, or Binungulosa; and Ruminantia, as they chew the cud. They have no true incisor teeth in the upper jaw, which, with their fourfold stomach, their ruminant propensity, and their cloven hoofs, will strongly characterize this natural group. The subdivisions of this race are formed by the presence or absence of canine teeth, the possession or non-possession of horns, or antlers, and their deciduous or persistent nature: but this is extraneous to our present purpose. The spurious canines of the camel, and its transitional hoofs, would seem to connect it with the other types, and form a link with the hoofed and the claw-footed beasts.—(*Vide* Table, p. 353.)

The horse and its allies, the hemione, the ass, the zebra, and the quaccha, the varieties of which are multitudinous, although they have mostly been referred to a single genus, and the chief of them even to a single species, will form the last race of the hoofed quadrupeds; the indicial character of which will be the possession of true incisor teeth in both jaws; each extremity terminates in a single greatly developed digit encased in an undivided hoof: the lateral toes may be seen as mere abortions. These animals do not ruminate. CABALLINÆ, Jumenta, Solipeda, Solidungula, Monungulosa, Exarmenta, are the various synonymes which different considerations might, with almost equal justice, apply to this race of beasts; the first is that which may be probably preferred. (*Vide* Table, p. 352.)

To pursue this analysis through all the kinds, the genera, and the species; or to critically examine how far many of the genera would admit, and, indeed, require subdivision, would far exceed the scope of the present essay; the following tables must now suffice: the developement of each kind would, in itself, form matter for an especial dissertation. All that was here proposed has been to indicate, and that in outline only, the general connexions of the kinds, the races, and the types of quadrupeds: and, as far as may be practicable, to render consentaneous popular and scientific terms. The uncouth and elaborate titles, with which too many genera and species are encumbered and disguised, might often in like manner be most advantageously reformed, and their magniloquent epithets be changed for other more simple and familiar names: but this would anticipate a future consideration, and involve a question of too great extent to be compatible with the present form and stage of these outline indications.

The genera of Linnæus, however, it may be remarked, here as elsewhere, are rather associations of secondary than of primary groups of species; they are, in fact, rather kinds than genera: *i.e.* rather a group of genera than a genus. His great ability and consummate skill were chiefly shewn in forming these general associations; and, in the ample range which his powerful mind took of the whole of nature, what wonder is it that, when much fewer individuals of each kind were known, many genera truly distinct were coalesced; and these sub-genera modern zoologists must, by degrees, distinguish and separate as such: yet, not to lose the advantage of his comprehensive scheme, retaining, as far as they are correct, his genera as the next larger groups or kinds. Thus his Canis, Felis, Ursus, Mus, &c. form our Canidæ, Felidæ, Ursidæ, Muridæ; and in like manner of the rest. Again, his genus Canis, which gives name to the kind, forms its sub-sections into genera, as Canis, the true dog; Vulpes, the fox, the hyæna, &c. Mus, also, which becomes Muridæ, gives as its sub-genera, now genera, Mus, the mouse and rat; Arctomys, the marmot; Cricetus, the hamster; Myoxus, the dormouse, &c. &c.

Type, QUADRUPEDA (Quadrupedes). Quadrupeds, truly four-footed beasts. Mammalious, ditrematous; furnished with four well-developed and freely-exserted limbs, terminating in feet.

Districts, UNGUIPEDATÆ (Digitate, or nailed, *i.e.* Unguipedate quadrupeds). Feet terminating in distinctly developed toes, or in digits less freely exserted, armed with nails: no abdominal pouches.

PRÆCOCINATÆ (Præcocinates, Marsupiatæ, Marsupiates, Mastothecatæ, Mastothecates, sacculate or teat-pouched beasts). Destitute of true placenta? The females parturient prematurely; generally furnished with a marsupium. Marsupial bones in both sexes.

CORNIPEDATÆ (Cornipedates, or hoofed quadrupeds). All the extremities terminating in toes more or less numerous, and more or less developed, but the digits enclosed in hoofs: no abdominal pouches.

UNGUIPEDATÆ,

Races, FERÆ, Plenidentia, or Tridentulæ; savage beasts. Three sorts of teeth—incisors, canines, and molars; more or less distinct; always in one, generally in both jaws.

GLIRES, Rodentia, or Bidentulæ, Liberæ; free beasts. Two sorts of teeth only, incisors and molars; never canines; a distinct diastemma in their stead: the incisors signally protruded, and chisel-shaped.

Subferinæ, Semiferæ, Edentia, Edentula, or Exincisoria; wild beasts. Teeth various, sometimes none; occasionally canines and molars, sometimes molars only, often none; almost never any incisor teeth; and the other teeth often without roots.

Præcocinatæ,

Races, Feridentiæ, Feridentulæ, Ferídents; three sorts of teeth in each jaw: chiefly entomophagous.

Semidentiæ, Semidentulæ, Semidents; three sorts of teeth in the upper jaw; only two in the lower, the inferior canines being absent: chiefly carpophagous.

Gliridentiæ, Gliridentulæ, Glirídents. Two sorts of teeth only; no canines in either jaw: chiefly phyllophagous.

Cornipedatæ,

Races, **Belluinæ**, Egreges, Armenta, Multungula, Subungulosa; Belluines, Outherds, or Hordes. Three to five toes on each foot, more or less equally developed, and included in a subungulate horny case. Not ruminant. Teeth various.

Pecora, Bisulca, Binungula, Ruminantia; Cattle or Pecudines. Cloven hoofs; no true incisors in the upper jaw. Stomachs more or less numerous and complex: ruminant.

Caballinæ, Jumenta, Exarmenta, Solidungula, Solipeda, Monungulosa: Caballines, Cavalry, Stud, Troops. A single greatly developed digit, with lateral abortive toes. A single undivided hoof: true incisors in both jaws: not ruminant.

Type.	*Districts.*	*Races.*
QUADRUPEDA, Quadrupeds.	Unguipedatæ, Unguipedates.	Feræ, Savage beasts. Glires, Free beasts. Subferinæ, Wild beasts.
	Præcocinatæ, Marsupiatæ. Marsupiates, Præcocinates.	Feridentiæ, Opossum race. Semidentiæ, Potorace. Gliridentiæ, Kangarace.
	Cornipedatæ, Cornipedates.	Belluinæ, Belluines. Pecora, Cattle. Caballinæ, Caballines.

Race.	*Kinds.*	*Genera.*	*Species.*	
Feræ Plenidentiæ. Savage Beasts: Ferines.	Canidæ, Dog-kind.	Canis	Familiaris,	Dog.
		? Lupus	— —	Wolf.
		Vulpes	Communis,	Fox.
		— —	Lagopus,	Arctic Fox.
		Fennecus	Brucei,	Fenneck.
		Lycaon	Tricolor,	Lycaon.
		Hyæna	Striata,	Striped Hyena.
		— —	Crocuta,	Spotted Hyena.
	Felidæ, Cat-kind.	Acinonyx	Venator,	Guepard.
		Felis	Tigris,	Tiger.
		— —	Pardalis,	Panther.
		— —	Catus,	Cat.
		? Leo	Africanus,	Lion, African.
		— —	Asiaticus,	Lion, Asiatic.
		Lynceus	Chaus,	Booted Lynx.
		— —	Caracal,	C. Lynx.
	Mustelidæ, Weasel-kind.	Putorius	Fœtidus,	Fitchet.
		— —	Vulgaris,	Weasel.
		Mustela	Furo,	Ferret.
		— —	Foina,	Marten.
		— —	Zibellina,	Sable.
		Mephitis	Striata.	Skunk.
		Lutra	Fluviatilis,	Otter.
	Viverridæ, Civet-kind.	Viverra	Civetta,	Civet.
		— —	Zibetha,	Zibeth.
		Suricata	Capensis,	Cape Sur.
		Genetta	Communis,	Common Genet.
		Herpestes	Ichneumon,	Egyptian Ichneumon.
		— —	Mungos,	Indian Ichneumon.
	Ursidæ, Bear-kind.	Gulo	Arcticus,	Wolverene.
		— —	Larvalis,	Masked Glutton.
		Meles	Taxus,	Badger.
		Ursus	Arctos,	Brown Bear.
		— —	Americanus,	Black Bear.
		— —	Maritimus,	White Bear.
		Prochilus	Hirsutus,	Shaggy Prochil.
		Procyon	Lotor,	Racoon.
		Nasua	Rufa,	Red Coati.
		— —	Fusca,	Coati Mondi.
	Talpadæ, Mole-kind, or Soricidæ, Shrew-kind.	Talpa	Europæa,	Common Mole.
		Chrysochloris	Capensis,	Cape Mole.
		— —	Rufa,	Red Mole.
		Condylura	Radiata,	Rayed.
		Tapaia	Tana,	Tapaia.
		Scalops	Canadensis,	Canad. S.
		Sorex	Araneus,	Shrew-fœtid.
		— —	Constrictus,	
		— —	Lineatus,	
		Mygale	Moscovitica,	Musk Shrew.
		— —	Pyrenaica,	Pyrenean.
	Erinacidæ, Urchin-kind.	Centenes	Semispinosus,	Tenrec.
		— —	Setosus,	Bristly C.
		— —	Spinosus,	Spiny Tendrac.
		Erinaceus	Europæus,	Urchin, or Hedgehog.
		— —	Malaccencis,	Malacca.
		— —	Auritus,	Long-eared.

Race.	*Kinds.*	*Genera.*	*Species.*	
GLIRES, RODENTIA, LIBERÆ: GNAWING BEASTS, GLIRINES.	HYSTRICIDÆ, PORCUPINE-KIND.	Hystrix	Cristata,	Crested P.
		— —	Macroura,	Rice-tailed.
		Erithizon	Dorsatus,	Erithizon.
		Sphiggurus	Villosus,	Hairy Sphig.
		— —	Spinosus,	Spiny Sphig.
		Sinætherus	Cuandu,	Cuan.
	LEPORIDÆ, HARE-KIND.	Lepus	Timidus,	Hare.
		— —	Glacialis,	Snowy Hare.
		— —	Cuniculus,	Rabbit.
		Lagomys	Alpinus,	Pika.
		— —	Pusillus,	Calling Hare.
		— —	Ogotona,	Ogotone.
	CAVIADÆ, CAVY-KIND.	Hydrochærus	Capybara,	Capyb.
		Cælogenus	Fulvus,	Paca.
		— —	Subniger,	Brown Paca.
		Cavia	Cobaya,	Cavy.
		Dasyprocta	Acuti,	Agouti.
		— —	Acuschy,	Akouchy.
	SCIURIDÆ, SQUIRREL-KIND.	Sciurus	Vulgaris,	Common Squirrel.
		— —	Cinereus,	Grey Squirrel.
		Cheiromys	Madagascarensis,	Aye Aye.
		Sciuropterus	Volucella,	Polatouche.
		Pteromys	Petaurista,	Taguan.
	MURIDÆ, MOUSE-KIND. OR RATTIDÆ, RAT-KIND.	Sphermophilus	Citillus,	Souslik.
		Arctomys	Alpinus,	Alp. Marmot.
		Pedetes	Capensis,	Cape Pedetes.
		Bathyergus	Capensis,	Cape Bathyerg.
		Dipus	Sagitta,	Jerboa.
		— —	Jaculus,	Siberean.
		Gerbillus	Indicus,	Indian Gerb.
		Aspalax	Typhlus,	Blind Zemni.
		Cricetus	Vulgaris,	Common Hamster.
		— —	Migratorius,	Wandering.
		Mus	Rattus,	Black Rat.
		— —	Decumanus,	Norway Rat.
		— —	Musculus,	Common Mouse.
		— —	Sylvaticus,	Field Mouse.
		— —	Minutus,	Tiny Mouse.
		Hydromys	Chrysogaster,	Yellow-bellied H.
		— —	Coypus,	Chilese.
		Myoxus	Avellanarius,	Common Dormouse
		— —	Nitella,	Garden Lerot.
		— —	Glis,	Fat Loir.
		Echimys	Cristatus,	Crested E.
		Lemmus	Terrestris,	Lemming.
		— —	Norvegicus,	Norway L.
		Arvicola	Arvalis,	Field Arvicole.
		— —	Amphibia,	Water Rat.
	CASTORIDÆ, BEAVER-KIND.	Ondatra	Zibethica,	Musquash.
		Castor	Fiber,	Beaver.

Race.	*Kinds.*	*Genera.*	*Species.*	
SUBFERINÆ EDENTIA, SUBFERINES: WILD BEASTS.	ACHEDÆ, SLOTH-KIND.	Unaus	Didactylus,	Unau Sloth.
		Acheus	Tridactylus,	Ai Sloth.
		Megatherium	— —	Fossil Sloth.
	TATUSIDÆ, TATU-KIND.	Daysypus	Apar,	Apara.
		Tatusia	Peba,	Peba Tatu.
		— —	Hybrida,	Armadillo mule.
		Priodontes	Giganteus,	Giant Tatouhou.
		Chlamyphorus	Truncatus,	Mantled Tatouhou.
		Orycteropus	Capensis,	Cape Oryc.
	MANIDÆ, MANIS-KIND, or ANT-EATERS.	Myrmecophaga	Jubata,	Tamanoir.
		— —	Tamandua,	Tamandua.
		Manis	Brachyura,	Short-tailed Pangolin.
		— —	Macroura,	Long-tailed Pangolin.
		— —	Javanicus,	Javanese.

Dist.	*Races.*	*Kinds.*	*Genera.*	*Species.*	
PRÆCOCINATÆ. PRÆCOCINATES. EXPLACENTARIA. MARSUPIATÆ.	FERIDENTIÆ, OPOSSUM-RACE.	DIDELPHIDÆ,	Didelphis	Virginiana,	Virg. Opossum
			— —	Cancrivora,	Crab-eating
			— —	Opossum,	Carigueia
			Monodelphis ?	Dorsigerens,	Touan
			— —	Brachyura,	Short-tailed T.
			Cheironectes	Palmata,	Yapock.
		DASYURIDÆ,	Dasyurus	Ursinus,	Ursine or native Devil
			— —	Macrourus,	Long-tailed
			Thylacynus	Striatus,	Zebra-Thylacyne
			Phascogale	Pennicillatus,	Brush-tailed P.
			— —	Minimus,	Little
			Perameles	Nasuta,	Long-nosed.
		PHALANGISTIDÆ,	Phalangista	Maculata,	Spotted P.
			— —	Cookii,	Cook's
			Balantia	Lemurina,	Lemurine
			Petaurista	Sciurea,	Squirrel-like
			Acrobata	Pygmæa,	Dwarf.
	SEMIDENTIÆ, POTO-RACE.	KOALIDÆ,	Potorus	Murinus,	Potoroo
			— —	Peronii,	Peron's
			Koala	Subiens,	Climbing K.
	GLIRIDENTIÆ, KANGA-RACE.	MACROPIDÆ,	Kangurus	Labiatus,	Larger Kang.
			— —	Fuliginosus,	Sooty Kangaroo.
			— —	Rufus,	Red Kangaroo.
			Macropus	Ulabattus,	Bush Kangaroo.
			Halmaturus	Elegans,	Elegant Kang.
		VOMBATIDÆ,	Phascolomys	Vombatus,	Wombat.

CORNIPEDATÆ, UNGULOSA:
CORNIPEDATES, HOOFED OR HOOF-FOOTED QUADRUPEDS.

Races.	*Kinds.*	*Genera.*	*Species.*	
Belluinæ, Belluines, Egreges, Hordes.	Elephantidæ, Elephant-kind.	Elephas	Indicus,	Indian Elephant.
		— —	Africanus,	African.
		Mammuthus	Borealis,	Fossil Mammoth.
		— —	Meridionalis,	
		Mastodon	Giganteum,	Gigantic.
		— —	Angustidens,	Lesser.
	Hippopotamidæ, Hippopotamus-kind.	Hippopotamus	Capensis,	C. Hippo.
		— —	Senegalensis,	S. Behemoth.
		— —	Antiquus,	Fossil.
	Rhinoceroscidæ, Rhinoceros-kind.	Rhinoceros	Indicus,	Indian R.
		— —	Sondaicus,	Sondic.
		— —	Sumatrensis,	Sumatra.
		— —	Africanus,	African.
		Elasmotherium	Fischerii,	
	Tapiridæ, Tapir-kind.	Tapirus	Americanus,	Am. Tapir.
		— —	Malayanus,	Malayan T.
		Lophiodon	Tapiroides,	Fossil.
		Palæotherium	Cuvierianum,	Fossil.
	Suidæ, Pig-kind or Swine.	Sus	Aper,	Wild Boar.
		— —	Scrofa,	Domestic Hog.
		— —	Priscus,	Fossil.
		— —	Larvatus,	Masked Boar.
		— —	Indicus,	Indian.
		Phascochærus	Æthiopicus,	Æthiopian.
		Babyrussa	Indica,	Indian B.
		Dicotyles	Torquatus,	Patira or Peccary.
		— —	Labiatus,	Tajassu.
Caballinæ, Caballines, Cavalry or Stud.	Equidæ, Horse-kind.	Asinus	Vulgaris,	Common Ass.
		— —	Zebra,	Zebra.
		— —	Quaccha,	Quagga.
		— —	Montanus,	Dauw.
		Equus	Hemionus,	Hemione or Dziggtai
		— —	Caballus,	Horse.

CORNIPEDATÆ, UNGULOSA:—(*continued.*) CORNIPEDATES, HOOFED OR HOOF-FOOTED QUADRUPEDS.

Races.	*Kinds.*	*Genera.*	*Species.*	
PECORA, PECORINES, CATTLE, FLOCKS.	CAMELIDÆ, CAMEL-KIND.	Camelus	Bactrianus,	Camel.
		— —	Dromedarius,	Dromedary.
		Auchenia	Vicunna,	Paco.
		— —	Llacma.	Lama.
	MOSCHIDÆ, MOSCH-KIND.	Moschus	Moschiferus,	Musk Deer.
		— —	Memina,	Memina.
		— —	Javanicus,	Javanese.
	CERVIDÆ, DEER-KIND.	Cervus	Elaphus,	Stag.
		— —	Canadensis,	Wapiti.
		Alces	Europæus,	Elk, or Moose Deer.
		— —	Coronatus,	Crowned.
		Tarandus	Rangifer,	Rein Deer.
		Dama	Vulgaris,	Common Hart,
		— —	Maculata,	or Fallow Deer.
		Rusa	Hippelaphus,	Great Rusa.
		Capreolus	Pygargus,	Ahu.
		— —	Dorcas,	Roebuck.
	GIRAFFIDÆ, GIRAFFA-KIND.	Camelopardalis	Giraffa,	Giraffe.
	ANTILOPIDÆ, ANTILOPE-KIND.	Antilope	Cervicapra,	Common A.
		Gazella	Dama,	Swift Gazelle.
		— —	Pygarga,	White-faced.
		Tragelaphus	Sylvaticus,	Boschbock.
		Antilocapra	Furcifer,	Prong-horned A.
		Rupicapra	Hamulicornis,	Chamois.
	BOVIDÆ, OX-KIND.	Boselaphus	Pictus,	Nylghau.
		Catablepas	Gnu,	Gnoo.
		— —	Taurina,	Kokoon.
		Bos	Bubalus,	Buffalo.
		— —	Taurus,	Beeve.
		— —	Grunniens,	Yak.
		Ovibos	Moschatus,	Musk-Ox.
		— —	Pallantis,	Fossil-Ox.
	HIRCIDÆ, GOAT-KIND.	Capra	Hircus,	Goat.
		— —	Ægagrus,	Wild G.
		— —	Ibex,	Ibex.
	OVIDÆ, SHEEP-KIND.	Ovis	Tragelaphus,	Bearded Sheep.
		— —	Ammon,	Argali.
		— —	Montana,	American Arg.
		— —	Aries,	Common Sheep.

Chemical Observations. By Thomas Graham, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow.

1. On the Application of spongy Platinum to Eudiometry.

In explaining the action of cold spongy platinum in disposing the union of mixed oxygen and hydrogen gases, it seems necessary to suppose that hydrogen is really accendible at common temperatures, but that its point of accension is unnaturally elevated in ordinary circumstances when it is not in contact with highly-divided matter, just as the boiling point of water and other liquids is elevated in smooth glass vessels. This view may be correct, although it only shifts the difficulty; for we have still to explain why hydrogen, if so accendible, does *not* take fire at superior temperatures when out of contact with minutely divided matter. But there is an apparent analogy between the circumstances of the suspension of the combustion in the one case, and of the ebullition in the other, on which the mind can rest with some satisfaction.

Soon after the discovery of Dobereiner, it occurred to both Dr. Henry and Dr. Turner to apply the principle to the analysis of mixed gases. But they immediately found that hydrogen could not at all times be withdrawn from a gaseous mixture by the action of spongy platinum, although the required addition of oxygen was made, as the action of that substance was paralysed or entirely suspended by the presence of certain gases in the mixture. The following table of Dr. Henry's exhibits the sum of our information on this subject.

"The first column exhibits the number of volumes of each gas required to render one volume of an explosive mixture of hydrogen and oxygen (in the usual proportion of two hydrogen to one oxygen) uninflammable by the discharge of a Leyden jar; while the second column shews the number of volumes of each gas necessary, in some cases, to render one volume of an explosive mixture insensible to the action of the sponge, and in other cases indicates the number which may be added without preventing immediate combination.

"In the first column, the numbers marked with an asterisk were determined by Sir H. Davy: the remaining numbers in that column, and the whole of the second, are derived from my own experiments :—

One volume of explosive mixture was rendered incapable of being inflamed by electricity when mixed with	Effect of adding the same gases to one volume of explosive mixture on the action of the sponge.
About *8 vols of hydrogen	not prevented by many vols.
„ 6 „ nitrogen	ditto.
„ *9 „ oxygen	not prevented by 10 vols.
„ *11 „ nitrous oxide	ditto.
„ 1.5 „ cyanogen	prevented by 1 vol.
„ *1 „ carbonized hydr.	not prevented by 10 vols.
„ 4 „ carbonic oxide	prevented by ½ vol.
„ *0.5 „ olefiant gas	prevented by 1.5 vol.
„ *2 „ muriatic acid	not prevented by 6 vols.
„ 2 „ ammoniacal	not prevented by 10 vols.
„ 3 „ carbonic acid	ditto."

From other observations of Dr. Henry, carbonic oxide appears to retard the combustion of hydrogen, by taking the precedence of the latter in uniting with oxygen, from superior inflammability. This I found to be much more strikingly the case with sulphuretted hydrogen gas, which Dr. Turner found to suspend the action of the sponge, when present even in the most minute proportion. When mixed with oxygen, this gas slowly disappeared under the influence of a dried clay pellet, containing spongy platinum ; the hydrogen only uniting with the oxygen, and the sulphur being deposited in the ball, which was soon thereby rendered inactive, but not till it had destroyed two or three hundred times its bulk of sulphuretted hydrogen in the course of twenty-four hours. In a mixture of sulphuretted hydrogen, hydrogen and oxygen gases in equal volumes, the oxygen united in twenty-fours to the hydrogen of the sulphuretted hydrogen, nearly to the entire exclusion of the free hydrogen ; but the union of the last with the remaining oxygen was determined in a few seconds by throwing up a fresh platinum ball.

Sulphurous acid gas is as efficient in this way as sulphuretted hydrogen (Turner) ; yet, upon trial, the sponge had no effect in determining the union of oxygen and sulphurous acid, even with the presence of moisture.

Olefiant gas in my hands was at first as powerful in preventing the combustion of explosive mixture as it was found to be by both Henry and Turner; but on washing that gas more sedulously with caustic potash, its interference was found to depend on a trace of impurities, for the ball always acted on explosive mixture within a few minutes, however largely diluted with this gas, if properly purified. Indeed, I had frequent occasion to separate hydrogen from olefiant gas, and found cold spongy platinum most effectual for the purpose. Neither did sulphuric acid vapour retard the action of the platinum; indeed, the other allowed the action to go on so rapidly, that from the elevation of temperature it was itself slightly acted upon, carbonic acid always appearing. The same was the case with the vapours of naphtha and the essential oils.

The action of these gases here, therefore, is unlike the action of the same gases and vapours in protecting phosphorus from oxidation.

The influence of sulphurous acid and sulphuretted hydrogen is not impaired by diminishing the barometric pressure.

2. Crystallization of Barley-Sugar.

The change in appearance, arising from crystallization, which sticks of barley-sugar undergo in keeping, is always instanced as a case of crystallization occurring in a solid body, without solution, and independently of external agents. The barley-sugar certainly does not then become a hydrate; and probably at the completion of the change is exactly of the same weight as before it began. But from an observation I have made, it would appear that the presence of a little moisture is necessary for the change, and probable that every portion of barley-sugar which suffers this change has been successively loosened and held in solution by that small portion of water, which begins to act on the outer surface of the stick and travels inwards.

Two fresh sticks of barley-sugar, dry and transparent, were introduced at the same time into separate phials; one of them with a stick of caustic potash, and the other by itself, corked up, and laid in a drawer. The barley-sugar, in company with the caustic potash, which would preserve it perfectly dry, did not undergo the slightest alteration in six months, but remained as transparent as at first. The barley-sugar in the other phial was scarcely altered during the first four months; but during the last two months, which were colder and damper, it became opaque on the surface, and the crystallization thereafter was propagated inwards to a considerable depth.

The effect of a small quantity of moisture in enabling solid amorphous matter to crystallize was observed very distinctly in the case of another substance. A quantity of sulphate of soda was rendered anhydrous by heat, and became a heavy powder. Placed in a confined atmosphere, kept purposely humid, the powder slaked like lime, swelling to several times its original bulk. It regains in two weeks its usual combined water (twelve atoms), and was then dry, and not in the slightest degree crystalline. Two days afterwards the powder was found a mass of crystals of the usual form of sulphate of soda, so dry as not to adhere to the blade of a knife; and it was not till after weighing that I satisfied myself of the presence of uncombined moisture among the crystals nearly to the extent of an additional atom. Here a small quantity of water allowed the powdery particles to right themselves, and adopt a crystalline arrangement, which they were incapable of assuming without it.

3.—Detection of Arsenic.

Perhaps no greater degree of certainty is desirable in the recognition of arsenious poisons than is attained by the reduction to the metallic state with subsequent oxidation; and certainly the addition to the usual suite of operations, which is the object of this notice, did appear, when first suggested to me by Mr. Clark of Glasgow, as a thing worth trying, an over-refinement, should it be found to be practicable. But, on ascertaining the extreme facility of the proposed process by actual trial, a view of the absolute certainty of the demonstration which it clenches completely altered my first opinion.

We usually stop on recovering the arsenious acid in minute crystals in the upper part of the tube. Scratch the glass tube with a file below the crystalline crust, and break it off. The upper part of the tube containing the supposed arsenious acid, and nothing else, may be boiled for a few seconds with a drachm or two of distilled water, to which a drop of caustic ammonia has been added. A solution is obtained, which, after being acidulated in the slightest degree by pure nitric or acetic acid, and divided into several portions on watch-glasses, may yield the most distinct and characteristic indications with the three best liquid tests, ammonio-nitrate of silver, ammonio-sulphate of copper, and hydro-sulphuret of ammonia.

The best proof I can give that there is no difficulty in this application of liquid tests is, that it has succeeded with every one of my practical pupils, setting out with the reduction of a quantity of the sulphuret of arsenic which never exceeded one-tenth of a grain, although it was in general their first attempt to test for arsenic.

The process for demonstrating the presence of arsenic seems, therefore, to be finally perfected by this unexceptionable application of the liquid tests, devised by Mr. Clark.

4.—CHROME ORANGE.

It is singular, that, although no other colour has been so much run upon for a couple of years in cotton yarns, no account of the mode of raising this beautiful tint, so far as I can learn, has hitherto been published; yet the process is universally known, and followed by dyers.

The first object is to procure upon the yarns a good body of chrome yellow, of the ordinary and familiar tint of chromate of lead. For this purpose, the goods are well charged with protoxide of lead, which is done by dipping them in solution of acetate of lead, and then decomposing the salt by lime-water, of which the lime takes the acetic acid, and leaves the oxide of lead in the cloth. Every trace of lime must then be got rid of by washing.

It is necessary to have nothing but oxide of lead in the cloth; for, with acetate or nitrate of lead as the mordant, the colour will be uneven. The goods are then passed through a bath of bichromate of potash, which instantly strikes the chrome yellow with the oxide of lead.

The orange is raised by throwing the goods so prepared into lime-water at or near a boiling heat. Lime, at that temperature, appears to be capable of partially decomposing the chromate of lead, taking half the chromic acid from a greater or less portion of that salt, and reducing it to the state of dichromate of lead.

The dichromate of lead is itself of a full red colour, and is best prepared according to the original process of Mr. Badam's (*Annals of Philosophy*, N. S. vol. ix.), by digesting a solution of the yellow chromate of potash upon carbonate of lead at a boiling temperature, in the proportion of one atom of the former to two atoms of the latter, stirring up the solid matter very frequently, as the action is far from energetic.

Caustic potash likewise converts the chromate of lead into the dichromate; but, from its strong disposition to dissolve the oxide of lead, as well as to withdraw chromic acid, did not answer with us for forming the dichromate.

It occurred to Mr. R. Ruathen, while engaged with this subject in my laboratory, to try if the dichromate would stand the glazing heat of a potter's kiln, which it was found to do, and to form a pretty good red on ordinary kinds of stoneware.

Geological Survey of the Island of Jersey. By Lieutenant Nelson, *Royal Engineers, Corresponding Member of the Plymouth Institution.*[*]

THE Jersey rocks may be referred to three classes, that of the argillaceous schists, that of such as are distinguished by the presence of felspar, and lastly, that of the breccia, composed of both.

The felspar rocks overlay the breccia and argillaceous schist, alternating with the last, which is also overlaid by the breccia.

ARGILLACEOUS SCHIST.

Beginning at L'Etac in St. Ouen's bay, a remarkable junction is observable in the little bay *a*, where the sienite joins the schist, and penetrates it in large irregular veins. About a mile to the south is the mouth of a remarkable valley, at the bottom of which, for nearly its whole length, lies the line of separation between argillaceous schist and the sienite, which last is obviously incumbent: a small portion of the former crosses the mouth at *b*, and comes sharply to the foot of the latter, without the slightest appearance of intermixture. Proceeding south-east from the head of the valley to the point *c*, where the two main branches of the St. Mary's valley separate, another spot of junction is obtained, the rocks observing the same order. *d* is in the St. Laurence valley, at the reservoir for the mill, one mile south of St. John's. From *d*, going south and passing *e*, you arrive at the Town Mills *f*, in the St. Helier's valley; at about 60 yards from the mill, the green porphyry overlies the argillaceous schist, in the most decided manner; this last re-appears at *g*, on the south side of a slight hollow, close to Mr. Ingouville's, and so continues until it disappears under a dark greenstone at *h*; on the eastern side of the hill it buries itself finally under the marshes. At Gallow's Hill, just where the St. John's road ascends the brow at *i*, it is distinctly seen underlaying the green porphyry, as this last stretches out towards Elizabeth Castle. Coasting St. Aubin's Bay, until you arrive at the valley *i'*, to the south of, and immediately underneath the St. Aubin's signal staff, you proceed northward until you reach the elbow *j* of the St. Aubin's valley, the stream of which crosses the line of separation of the argillaceous schist and sienite, in two or three places; the last point where I could trace the junction between the two rocks is at *k*, about 100 yards below the New-road Bridge (Pont Marqué). The two rocks can now still be traced to within a short distance of each other until you reach *l*, the last appearance of the sienite. The sands of the Quenvais render the line from *k* to *l* somewhat indistinct. Though the slate underlies the sienite, it frequently pervades the latter in conformable beds, particularly observable on the north-west coast; at Portelet bay; at the southern extremity of Fort Regent; and, above all, at Gouray, where the slate bed is, at least, 100 feet in breadth; small isolated portions of the schist are also often found completely detached, although in the vicinity. There are fifteen such beds between *i* and Portelet bay, varying from about 2 feet in breadth to 30 feet, and afford a complete example of the slides and faults in the mining districts; the beds running about east and west, and the latter north and south.

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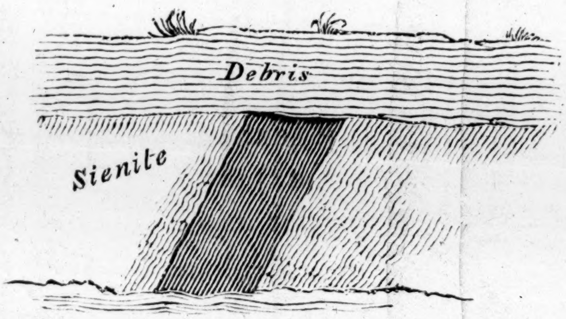
[*] The letters in this paper refer to a map, which we have not been able to prepare for insertion in this Number.

The comparative durability of the substances of the two rocks is singularly shewn by the invariable failure of the argillaceous schist before the action of the sea, having chasms and channels often of considerable depth; as is the case at the little insulated headland *n*, in Portelet bay, whereby a broad and deep dyke has been formed: the vein has also forked down at this point. At *o*, there is another similar chasm, at least 50 feet deep, and 20 wide; the walls of sienite are quite vertical.

So complete has been the removal of the argillaceous schist in these cases, that caverns 20 or 30 feet deep have been worn out in the cliffs where these veins first emerge; the ceiling of these is actually composed of the debris of the vallies above, shewing clearly a more daring hand than that of man in their excavation.



The angle of inclination of the strata varies from 15° to 60°, dipping generally west of north.

21* exemplifies the nature of the rock from the centre, eastward, and 22, 22 *a*, 22 *b*, 23 from the centre, westward; these specimens present all the prominent points of character in the argillaceous schist, as it changes from a mere shale to the subcrystalline granular and extreme variety (found mostly near St. Aubin's) that verges closely on grauwacké. I have, also, met with small specimens of the roofing slate at Greve-de-Lecq. In the St. Peter's valley, the new road, winding along the bottom, finely exhibits the structure of this rock, as it crosses the strata in every direction.

That of which 21 is a specimen, crumbles and shales away completely, from the action of a few years' sun and rain, (for there is but little frost in Jersey). This effect has been produced in a striking manner at *i*, on the north side of the St. John's road (see 24 and 25) in less than six years. Thin seams of black oxide of manganese, in *great abundance*, pervade the interstices between the planes of stratification.

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* The numbers refer to specimens sent by the author to the Museum of the Plymouth Institution.

Water is plentiful and excellent during summer and winter.

Felspar Rocks.

Under this head may be arranged felspar alone.

1. A very compact and intensely hard sort of a pink-grey colour, passing into

36. Another, equally hard, of a blue-green cast, but containing crystals of pale-buff felspar in abundance, forming a decided and beautiful porphyry.

26. 27. 28. A dark bluish green (externally, but dark grass green when cut and polished) porphyry, with the crystallized particles of a spicular form.

33. A light yellow ochre-coloured variety of No. 1, but more decidedly porphyritic.

10. An irregularly laminated and pink-red porphyry.

2. 4. 12. Ditto, containing concretions, sometimes nearly spherical (like those in the pisolite), in concentric coats, and internally radiated, at other times irregular: a curious rock.

3. A fine grained variety of 10.

5. A white variety of 2.

Felspar and Quartz.

43. A fine granular species.

Felspar and Hornblende (Greenstone).

15—39. 18—40. 19—44. Greenish-white felspar and hornblende in various proportions.

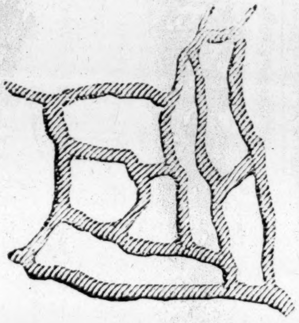
16 and 17. *Felspar* and *Schorl*.

13, 41, 42, 45, 52. *Felspar*, *Quartz*, and *Hornblende* (Felspar, red).

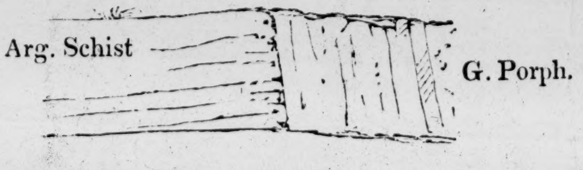
14. *Felspar, Quartz,* and *Green Mica* (Felspar, red).
50, 51, 53. *Felspar, Quartz,* and *Common Mica,* ditto.

FELSPAR ALONE.

1. This rock forms the sharp, prominent, and weather-worn columnar peaks along the coast from Tremont to the point *p* in Boluey bay, where it comes in contact with the breccia: in the same manner it runs from *r* to *s*, until it is overlaid by the sienite at Mont Orgueil. It seems to pass into two rocks; at *t* (in the Trinity valley) it forms peaks to 36; and all round the coast, as above pointed out, it invariably indicates 2, 3, and 10 below. Where exposed to the weather, its external surface, for about an inch in depth, sometimes breaks and cracks into facets, but without becoming soft.



26, 27, 28. I can only find these in the immediate neighbourhood of the argillaceous schist, as at Gallows Hill, round St. Saviour's, and at Gouray, accompanying the large bed*. It is the only one of the felspar rocks that in anywise intermixes with this last, which in this case assumes the form of a very compact writing slate, as a matrix containing dispersed but fairly mixed portions of the green specular porphyry. Its relative position, with respect to the argillaceous schist, as distinct from it, on the whole is shewn very decidedly at *f*, near the Town Mills.



It is a very imperfectly stratified rock, not homogeneous in composition, being, for the most part, full of cavities, which contain pulverulent black oxide of manganese, 32. This mineral penetrates it in every direction, giving it, when acted upon by the weather, a constant tendency to separate into small shapeless pieces.

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* Also in a vein of argillaceous schist near Noir Mont.

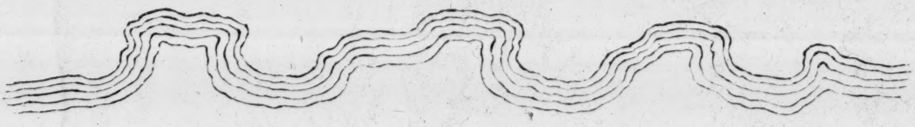
Carbonate of lime (nearly the only hitherto observed appearance of it in the island) is also found in this rock in small and irregular portions. 29, 30, and 31, shew the green porphyry in a state of incipient decomposition, after an exposure of a few years. 37 is the same rock, containing iron pyrites, in the neighbourhood of a mineral water, *z*, in St. Saviour's parish.

At *x* and *y* it passes into 36 and 33 respectively.

36 is found at *u* in the Trinity Valley, at V in Anne Port, at Bonne-nuit bay, and Havre Giffard.

33. A very abundant rock extending over the yellow space, dashed with yellow: it decomposes, especially when just under the surface of the ground, as in specimens 34 and 35. It often presents a white striated appearance when superficially decomposed, thereby shewing a near relationship to 10.

10. The structure of this rock is best exposed on the seacoast, where it appears as waved and striped, as if it had, in its soft state, been somewhat irregularly *worked* about.



It is pervaded by a large bed of argillaceous schist at Havre Giffard, and at the bay east of Belle Hougue, marked *y* (No. 11). When underground, but near the surface, the moisture decomposes it into its constituent and irregular laminæ, which crumble with great ease at the touch, and which become white when exposed to the sun in this state.

A reniform variety of felspar, with concentric coatings (No. 12), is found in small irregular masses among the hollows of this rock in St. Catherine's Bay.

3 and 2. From Boulay Bay, where they abound.

4. I am uncertain as to the absolute locality of this rock in

this island, although it is found at Guernsey. Hitherto I have been unsuccessful in obtaining it *in situ;* but, from the large masses near Vicart and at Boulay Bay, the parent rock can be at no great distance.

These large masses are agglomerates of the same bodies, only they are about the size and shape of potatoes of various dimensions, standing out in full relief, of the colour of 12, which appears to be only a variety of this rock, the chief difference being the regularity of the concretions; which last circumstance, I have however found in every degree, assuming Nos. 4 and 12 as extremes. It is used in the construction of piers, and in cases where large blocks can be best turned to account.

5. Apparently a white variety of No. 2, which it underlies, and of which it contains small portions. The concretions shew evident symptoms of 5, *a*.

Epidote, serpentine, and diallage are found in small quantities at Boulay Bay. At the mouth of the Trinity Valley, the bed of the stream contains small rounded pebbles, covered with a coat of the black oxide of manganese, nearly one-fortieth of an inch thick.

FELSPAR AND QUARTZ.

43.—From the neighbourhood of Noirmont and Le Corbière, where it is to be found in plenty.

FELSPAR AND HORNBLENDE.

15 is from La Motte, and not very abundant; it is taken from among 16 and 17 rocks; 18 and 19 are specimens of the whole coast of sienite, at the south-east of the island, beginning at Fort Regent, and reaching to Roque Berd; also from Sorel to the bay marked *x*, at the north of the island. This variety always assumes a reddish aspect (from the oxide of iron in the hornblende) at the surface; and the crust thus formed can be generally detached by a light and sharp blow. From the frequent inequality of disposition of the hornblende, this rock bears often a singularly mottled appearance, as at Elizabeth Castle, where the rock 38 may be well taken for a conglomerate, until this habit is known. Another example of this is given in 52. At the south-west point of the Hermit

Rock (Elizabeth Castle), this alternate variegation of the red felspar, with the very compact and dark-coloured greenstone, is disposed nearly in horizontal layers, while the regular strata of the rock stand at an angle of 70 or 80 degrees; the red layers *always* projecting from the weather-worn failure of those in which the hornblende predominates. (See *fig.* D, p. 374.)

In figures A, B, C, and from the circumstance of the red veins very generally constituting the matrix of the green, and the substance of the veins that traverse it, as well as that in figure C; the green portions are of various descriptions, it may be questioned whether the red and green sienite are absolutely coeval.

49 is a good specimen of the very compact dark greenstone just alluded to, although it was taken from a large vein between Grosnez and Plemon. This last is not quite horizontal, but continues about 7 feet thick, whilst it gradually rises along the face of the almost vertically stratified cliffs: upwards of 3 feet in depth have been decomposed and disappeared. This vein divides in one place, which reduces the thickness of the two parts to that of about 3 feet each, until they rejoin and proceed as before. The same substance frequently accompanies the veins of argillaceous schist.

40, from Elizabeth Castle, as in 39; which last, although but an accidental stone from the beach, corresponds to 44, which was taken from a vein from Portlet Bay; it is, I believe, the hardest rock in the island.

20, shews the ultimate state of decomposition of the hornblende and felspar.

An east and west vein of chlorite mica; traverses the hill where Fort Regent stands, 19 *a*.

FELSPAR AND SCHORL.

16 and 17, From La Motte; not much of it. I have seen specimens in which the schorl lay as minute, confusedly-scattered and divergent crystals in the felspar.

FELSPAR, QUARTZ AND HORNBLENDE.

The felspar in this variety is red, and this association with quartz and hornblende, forms by far the most extensive rock in the island. In the south-east it lies within the space marked out by Mount Orgueil, Granville church, *a'*, and La Hougue Bie. In the south-west there is hardly any other rock (except 43), and it is generally of a large, coarse-grained description.

The well-known Mount Madou, quarry 52, is the last point at which this sienite shews itself, where it overlays the pink-red porphyry, as decidedly as it does at Mount Orgueil. It reaches also from l'Etac to half-way between Greve-de-Leiq and Plemont, and from X Bay to Mount Madou. At X Bay all the three constituents are in large grains, which apparently present the most decomposable description of sienite. I have seen the hornblende fairly washed out by the sea, leaving a frail honey-combed skeleton of quartz and felspar.

Although this rock alternates with the argillaceous schist, yet there appears the utmost jealousy in keeping itself quite distinct; 46 was chipped from the junction at *a*, (l'Etac). (See *Fig*. E, p. 39). 47 shews the quartzose nature of it as as composing the vein, and which becomes gradually though decidedly sienite at about 20 feet from the point where it enters the argillaceous schist. At *a* are numerous veins of white quartz (48), 6 or 8 inches thick.

FELSPAR, QUARTZ AND CHLORITE MICA.

14, Found in the space contained by Le Hoc St. Clement's, Granville church, and thence eastward.

FELSPAR, QUARTZ AND COMMON MICA.

53, From half-way between Plemont and Greve-de-Leiq to X Bay. At Greve-de-Leiq there are two large veins (54) of mica 10 feet wide; close to, and parallel to one of them, is another vein of argillaceous schist.

With the exception of the green porphyry, all the preceding felspar rocks have a peculiarly columnar structure, especially Nos. 33, 36, 41, 42, 43; the finest example of this lays between Le Corbière and the western point of St. Brelade's Bay. They form hills of from 400 to 200 feet in height: the green porphyry no where exceeds 200.

The stratification, in general, is very perfect, though by no means so much so as that of the argillaceous schist. Fibrous malachite is found in small quantities in the green porphyry; the constant and plentiful occurrence of the black oxide of manganese, in the same, has been already noticed. There is a vein of clay-iron ore in X bay, and the oxide has been found in small detached pieces in different parts of the island. Sulphuret of iron is extensively disseminated in minute grains through the sienite and porphyries; hence, though water is abundant even in summer, it is not unfrequently contaminated by this mineral.

Few rocks surpass those of felspar as building materials, of the utmost durability; they are, however, frequently too hard to repay the expense of being worked as squared stones. At Fort Regent, all the ashler was obtained from near the Icho Rock; the copings, &c., *a*, from Mount Madou, whilst the rock on the spot only afforded rough materials for coarse work. St. Brelade's church (built 1111) is in existence to this day.

BRECCIA.

Extending from *p* in Boulay bay to *d* (a house half a mile from St. Martrés, bearing 7° west of the church), thence to *e*, on the Rozel road, about a quarter of a mile from the church: *f* is on the St. Catherine's road, about one-third of a mile from the Martello Tower, and *r*, the extreme point on this side, is nearly in the middle of the bay. It also appears at *d*, nearly in the centre of the island, interposed between the felspar and argillaceous rocks. It is a well stratified rock.

From *p* to Rozel Harbour the planes dip north, from the last mentioned point to La Coupe and Verclut, south; so that the turn of the saddle is somewhere in Rozel Harbour. The general height of these hills varies from 300 to 200 feet.

It is *overlaid* by the felspar rocks in all directions; and although formed of all the ingredients already mentioned, without any apparent order of seniority, they are of course to be referred to an earlier date.

I have observed five varieties.

Argillaceous pebbles		in an argillaceous matrix.
Sienitic	do.	
Porphyritic	do.	
Argillaceous	do	in a sienitic matrix.
Sienitic	do.	

6, 9 and 9 *a*, are illustrative of most of these when freshly cut: the argillaceous varieties of this rock are hard enough; but they all fail exceedingly on exposure to the weather, the matrix more perishable than the clay-pebbles it contains, which, however, betray their origin, by shaling and splitting after a short time. These last, as well as the sienite-pebbles, sometimes weigh several hundred weight, and are as thoroughly rounded as the small ones.

Sulphuret of iron and manganese still maintain their ground, the latter enveloping the pebbles with a thin film, and with the former penetrating the matrices (6), although I heard the best character, for quantity and quality, of the water of this rock.

Diluvium, or Sea Beach?

The rocks on the shore, from Rozel Harbour to Gouray at Le Hoc, under Fort Regent, from thence to the second Martello Tower in St. Aubin's Bay, from Blanc Pignon to La Carriere, and at L'Estac, emerge from the diluvial deposits lying at the bottoms of the vallies and round the bases of the sea-side hills, forming abrupt banks from 20 to 50 feet high, and invariably composed of the debris of the nearest rocks respectively.

The lower strata of these banks often consist of well-rounded pebbles: this fact is well-exemplified at the little bay *a*, at L'Etac, where the superincumbent debris is at least 50 feet high, and is a thorough mixture of the two rocks, which come into contact at this spot.

The hills are capped in general with a sandy clay (55) intermixed with debris of the contiguous hills.

The flat ground at the mouth of St. Peter's Valley is a complete morass, so that I could obtain no information respecting it; but from what I have observed at St. Aubin's Pier, as compared with that under Fort Regent, I have no reason to suppose that it varies in its nature from that of the soil in the St. Helier's Valley, and in the contiguous flats extending to near Mount Orgueil. Respecting these last, I have been able to procure the following sections:—

Well at Elizabeth Castle.

18.6 Sandy, fine clay, (see No. 54).
6.0 Debris, angular.
4.0 Pebbles }
4.0 Blue clay } containing the water.

Well at Fort Regent,

236 feet deep; all sienite; water soon found; but continued till 30 or 40 tons per day came in from all crevices and corners, not from any particular stratum.

West end of St. Helier's—sandst.

7.0 Sand.
10 to 15 A mixture of vegetable earth and stones.
8.0 Sandy clay, No. 54.
4.0 Blue clay.
8 to 12 Gravelly clay, in the lower 5 of this the gravel was clean, water-worn, and containing an indifferent water.

Centre of St. Helier's,
Mt. Le Capelain.

8.0 Sand.
16.0 Blue clay intermixed with strata of No. 54; in this, at 16 feet below the surface, hazle nut shells and a boar's tusk were found.
2.0 Turf.
2.0 Rounded gravel, mostly sienite. Water impregnated with sulphur. Rock.

Upper end of the Valley,
(near *g*.)

2 feet vegetable earth.
12.0 Sandy clay, No. 54.
16.0 Angular debris, clean, and containing abundance of water, but not of the purest quality.

Pier, on blue clay.

All the valley waters throw down a precipitate, adhering to the vessels in which boiled; and some of them are occasionally covered with a film of sulphur. By all accounts, the nature of the ground in the flats, that reach from Fort Regent to Grouville, bears a close resemblance to that which has been just described; except that water is more uncertain, and when found, generally brackish; sometimes 80 feet have been bored in vain, through sandy, blue, and whitish clays; and at other times water has been met with at 16 feet below the surface, as near St. Clement's Church.

SAND.

The flat grounds on the southern and western coasts are liable to the encroachments of the sand, blown over them by the winds: this is particularly the case in St. Owen's Bay, where a considerable tract (called Les Quenvais) has been thus ruined. Tradition (for no authentic records can be procured, as those of the Jersey States hardly reach to an earlier period than 1500) states, that an inroad of the sea overwhelmed a large extent of ground in this quarter; and that the sand, no longer kept in check by its old barrier (a wood of oak trees), swept over the space marked *q*, *q*, *q*, until checked by the St. Aubin brook; stumps of oak trees, generally in good preservation, are found lying in the sands near L'Etac. This circumstance of a stream of water, however inconsiderable, arresting the progress of the sands, is further illustrated at Greve de Lecq, and at St. Perranzabuloe, in Cornwall. Might it be turned to account at Bayonne, where the encroachment of the Dunes is so very formidable? The Quenvais originally extended as far St. Peter's Church, still controlled by the brook. Much, however, of this ground has been reclaimed by the persevering industry of the Jersey farmer. In one instance pointed out, a tract of sand-hills, only levelled five years since, now produces parsnips and mangel-wurzel of the largest size, and lucerne, in a soil of no greater depth than that of six inches, formed of vraic (sea weed) and such vegetable mould as could be collected. The black mould, overwhelmed by the sand, and forming the original surface of the ground, varies from two to three feet in depth. Some of it has been cleared and dressed with vraic, or lime, at the rate of four hogsheads per acre, but the expense has been found too great.

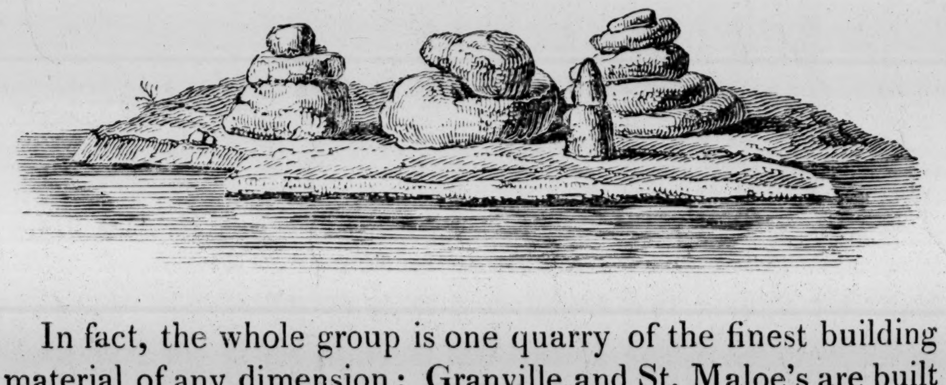
The soil on the island is generally very fertile; the best lies in the central parts included by a belt reaching about one mile inland from the sea-coast. Of this central tract, that on the argillaceous schist, by all that I could collect, is of a lighter cast, and, on the whole, inferior to that on the felspar rocks. The soil at Trinity ranks first; it is a stiff loam, which, for agricultural purposes, is ploughed up with sea sand and the gravel from the decomposed sienite. The tract on the green porphyry round St. Saviour's, that about La Hogue Bie and St. Clement's, are in the next best estimation.

CHAUZEY.

A GROUP of small islets and rocks, eight miles N. W. of Granville, about eight miles long and two and a half wide at an average. Whatever irregularity may be presented by the Jersey rocks as to the number and proportions of the granitic ingredients, those of Chauzey, though similar on the whole, differ distinctly from the above in the uniformity and more equal proportions of their constituents, in their unmanageable irregularity of stratification, in the rounded forms, and frequently gigantic size of the more or less detached blocks on the surface, and in the singular tendency of these last to scale off, when attacked by the elements, in coats concentric to their external and rounded surface.

This rock is almost invariably such a mixture of quartz, felspar, and mica, as is exemplified by Nos. 59 or 53. Sometimes, but comparatively seldom, the felspar and quartz assume a dark red colour, as No. 60. Another, although a minor point of difference, is, that this rock at Chauzey contains very numerous isolated portions of semi-transparent quartz, not exceeding perhaps 20 cubic inches, and also of closely aggregated mica, occasionally containing schorl (61): 59, *a*, is a specimen resembling grauwacké, from a block imbedded in 59. As to the stratification, it is very *marked*, often as much so as the newest rocks, but varying in every direction.

With reference to external form and size, the blocks have exactly the round boulderstone appearance, and often the arrangement of the Cheesewrings; sometimes the detached are nearly cubical, having a length of side of 12 feet.



In fact, the whole group is one quarry of the finest building material of any dimension: Granville and St. Maloe's are built of this stone. The Minquiers and remaining islets (on good authority) are composed of the same rock.

The want of leisure prevented my examining the rocks of Chauzey with the same minuteness as those of Jersey; and on the French coast, being still more limited, I was obliged to relinquish the second geological object, actual boundaries and extent, and confine myself almost exclusively to the primary one, order, as I found them between Coutances and Mount St. Michael.

Coutances itself stands on a slate hill, strata dipping south: proceeding south, and crossing the Soulle, you still find slate, but overlaid at the foot of the hill by quartz, pebbles, and a conglomerate (63) of red sandstone, in a matrix of the latter dipping west, and capped by a diluvial deposit of the same. One mile and a half on the direct road to Avranches, brings you to a species of siliceous oolite, of which the specimen 64 shews the largest grained sort that I could find; there was a finer kind very closely resembling, in texture, the regular calcareous oolite: in fact, a variety of sandstone. The broad top of the first hill, south of Coutances, is a heath, under the surface of which, to the depth of a few feet, debris of 64, imbedded in 67, lie on the parent rock, which, on the Granville road, and on the same hill, becomes 65, intermixed with 62, and alternating with it 67, and 68, dipping north. Still following the Granville road, at the foot of the above-mentioned hill, this red sandstone proves itself to be the old red sandstone by overlying limestone, 69, which, from its madreporites, (these I found in a large polished slab from Mount Martin) corresponds to the transition limestone at Plymouth. According to the geological map, in Coneybeare and Phillips, this rock, in Devonshire and North Wales, is found in long stripes, and it maintains the same character here; lying east and west, extending (according to the best information I could collect) a few leagues only inwards, and not exceeding two miles in breadth: 66, is obtained two leagues west of Coutances; I had not time to visit the spot.

At Hienville, this limestone is underlaid by another red sandstone, dipping east. Where this overlies the slate I cannot say, as the hill recedes from the road, which, further on, by having ascended the summit of the coast-range, affords no means of ascertaining the precise junction. This slate, as well as the limestone, dips north and west; it corresponds, in every respect, to the Plymouth transition slate in external structure, and internal composition: at Granville it becomes decided grauwacké, sometimes as 73, and the conglomerate [*] 72, when immediately at the peninsular on which the town stands, and at other times 70 and 71. At 73, it is intersected in every possible way by minute and very abundant quartz veins. These last, and the numerous pebbles of the same, occurring in the conglomerate, together with the very siliceous character of 73, prepares the way for what, perhaps, was the origin whence the veins were deposited, and the pebbles torn, *i. e.* quartz rock [†], 75, 76. This is found one mile south-east of Granville, on the south side of the valley, under the road to Avranches, in cliffs of such dimensions as to warrant the possibility of its having some extent, especially when the large deposits of the same, *en debris*, covering the flat hill-tops near Granville, are taken into consideration. It dips north and west, underlying the grauwacké slate, which completely intercepts it seaward, sometimes in the shape of 74, at other times as usual.

About half way between Granville and Avranches, the slate ceases, as it overlays the rock 77, 79. Avranches itself stands on a high abrupt hill, composed of this last and 78 (of which I am quite uncertain as to the extent). Following the road to Mount St. Michel, at the ferry, you again find slate (80), dipping south-east [‡], lying east and west, covered with a debris of mica slate, &c. 79 *a*. The mount itself is composed of 79 and 81, and as this last contains quartz, felspar, hornblende, and mica, there can be no objection to terming it granite. On the authority of my guide, from its external appearance, and correspondence of situation, I have good reason to think

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[*] I cannot leave this without pointing out how completely this conglomerate, in the siliceous extreme of the clay state, seems analogous to the Jersey breccia, as accompanying the argillaceous variety.

[†] This rock is, probably, at no great distance from the west of Jersey, (compare 22 *a*, and 22 *b*, with 70, 71, 73) as the beach at St. Ouen's Bay is, in a great measure, covered with its pebbles.

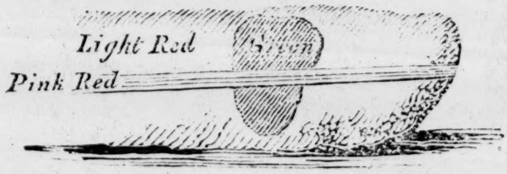
[‡] Apparently overlying the Avranches rock.

Tourbelaine consists of the same rock as Mount St. Michel: I regret that the state of the tide did not allow me a personal inspection.

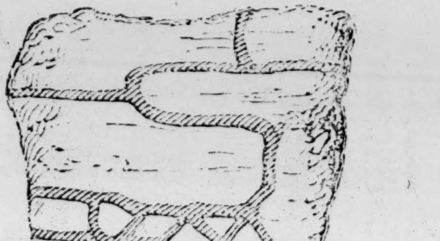
The coast-line to the north of, and in the vicinity of Granville, presents an interesting study of one mode of the natural and gradual conversion of vertical cliff into low and sloping hills. From A to B, the cliffs are generally precipitous enough, partly from having been scarped under the fortification, and partly naturally: at B the sand begins to form, not only an ordinary beach, but also an accumulation, as a glacis, 6, which thus immediately depriving the sea of access to the foot of the cliffs, (from which it not only tears out fragments, but carries them off,) and also forms a hollow, 7, which, sloping northward, forms an irregular, but on the whole, connected drain, *e*, of ponds, which (as in the Jersey Quenvais) entirely prevents encroachments of the same sand, that has already proved an efficient barrier to those of the sea.

Returning to B, and following the course of the hills, the now undisturbed ecroulements from the top of the cliffs, successively and gradually present themselves as the ramps $\left.\begin{matrix}1 & 2\\ 1' & 2'\end{matrix}\right\}\left.\begin{matrix}3 & 4\\ 3' & 4'\end{matrix}\right\}$ &c. until at about two miles from Granville the rock entirely disappears, under such a slope as 5 5′ 5″ covered with grass and a fine soil.

A

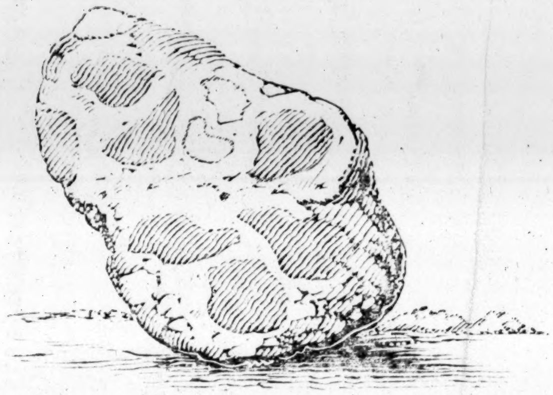


B



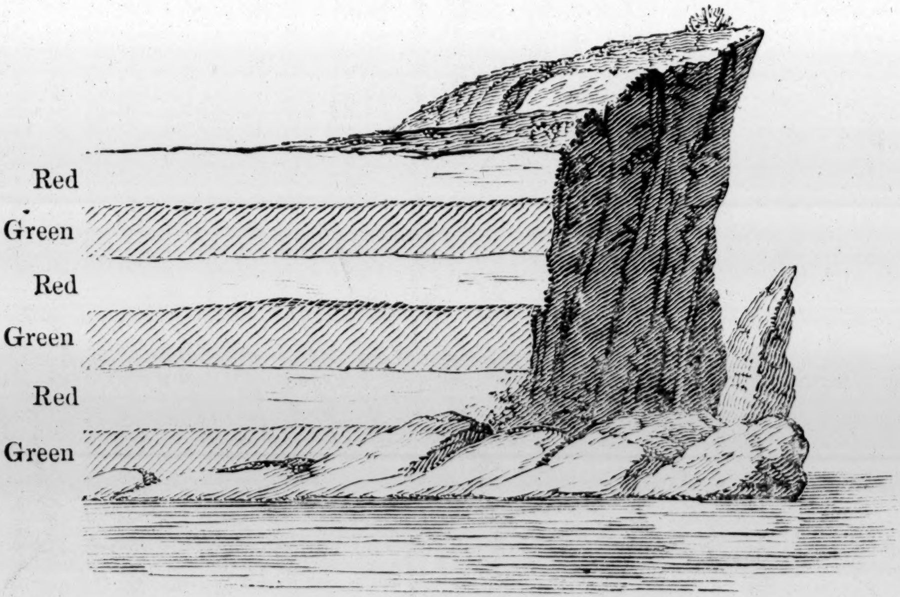
B, red veins in green.

C

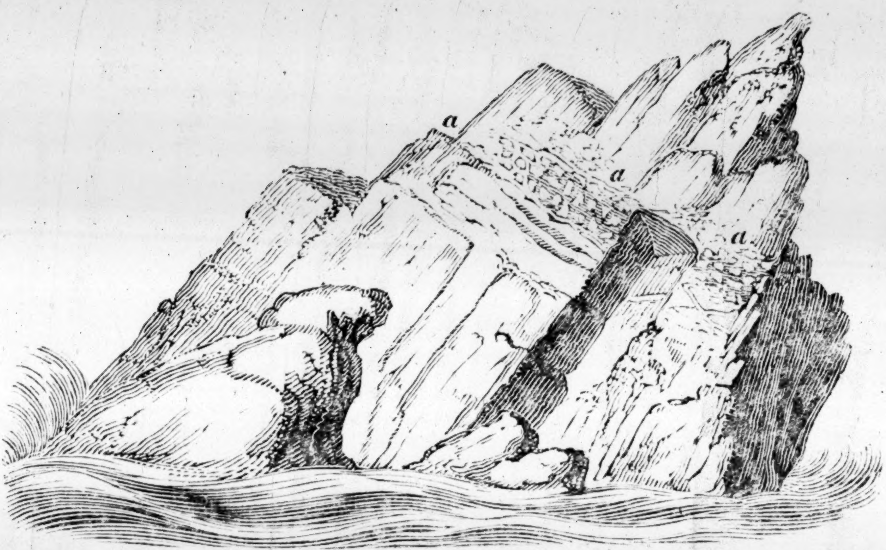


The shaded parts always slightly rounded, as if somewhat water worn; neither do they by any means bear the same aspect, although sufficiently so to conclude that they had a common origin.

D

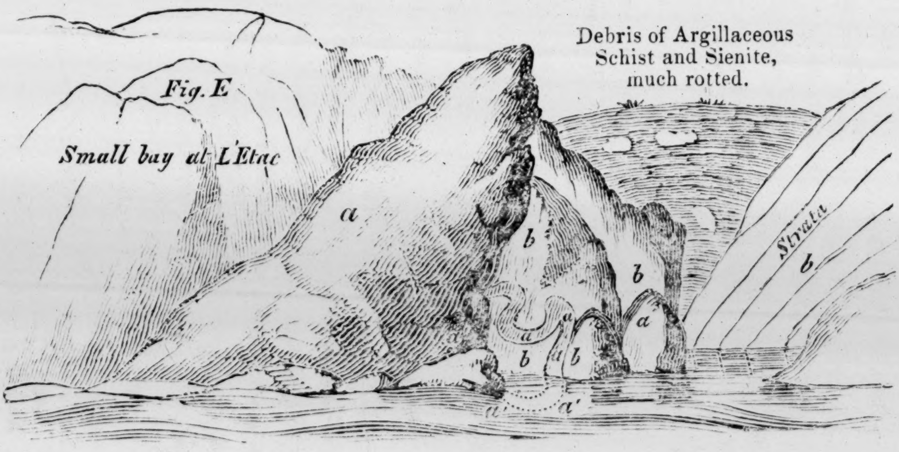


Care must be taken, in examining the above Figure (D), not to mistake the lines by which the Green is distinguished from the Red for cleavage lines.



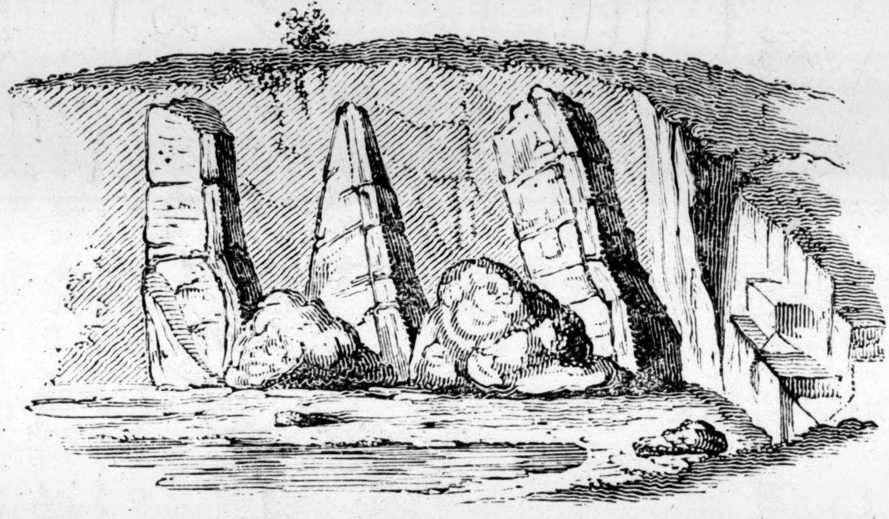
At *a*, the rock assumes the appearance of *fig.* B; the remainder well exemplified by C.

a, has precisely the look of having been shattered by percussion, but the fragments scarcely dislocated.



a, Sienite entering the Argillaceous Schist (*b*) by the vein *a' a' a'*.

F



Columnar appearance assumed by No. 36, in a quarry in the Trinity Valley.

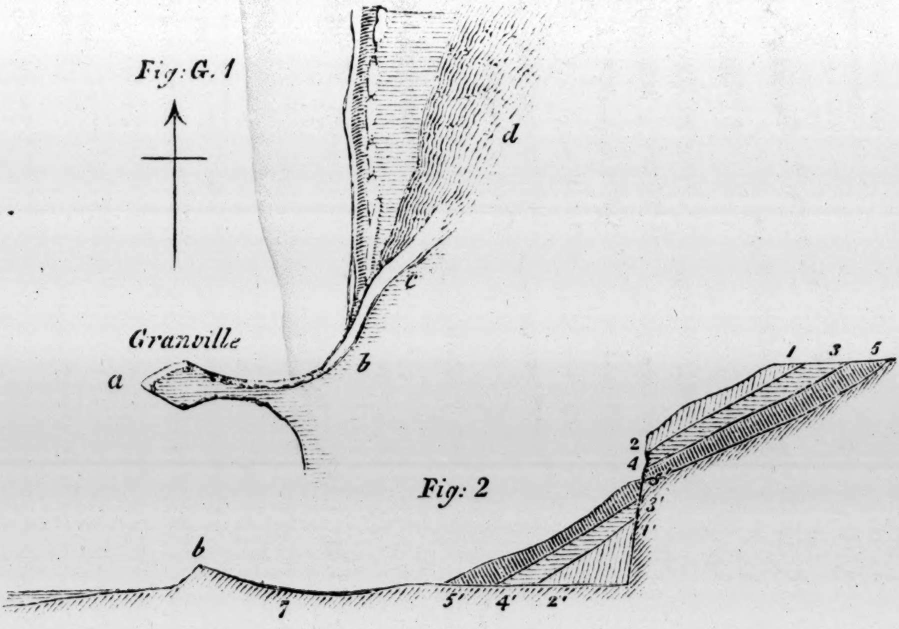


Fig. 2, is a general and considerably enlarged section of Fig. 1.

On Achromatic Telescopes.

Having perused, in the last number of the *Quarterly Journal*, Signor Santini's very scientific discussion on a new construction of the achromatic telescope, from which it would appear that the spherical aberration cannot always be destroyed by the separation of the lenses of the correcting glass, I beg leave to observe that this arises from the data upon which his calculations proceed being somewhat different from those which I adopted.

Signor Santini supposes that the foci of the two lenses of the correcting glass are exactly equal for mean rays. This can only take place when the thickness of these lenses is evanescent: when they are of a sensible thickness, the focus of the concave must be shorter than that of the convex, otherwise their foci cannot coincide, and the combination cannot act as a plane, but as a convex lens; and it is expressly stated in the paper which was read before the Astronomical Society, that to make allowance for the thickness of the lenses, I assumed the focus of the concave lens a quarter of an inch shorter than that of the convex. Our calculations, therefore, proceeding on the different suppositions of $9 + v = 0$, and $9 + v = 0{\cdot}25$ inch, have led to different results.

It is not necessary to limit the construction to the condition of the correcting lens acting as a plane; it is rather advantageous to render it slightly concave by making the difference of the foci of its component lenses somewhat greater than is necessary to compensate for the thickness of these lenses. This enables us, with the smallest possible curvatures, to create an excess of concave spherical aberrations, which can *always* be removed by reducing the aperture of the concave lens, and this is done by separating the two lenses.

I consider myself indebted to Signor Santini for the interest he has taken in the construction; and should be happy if so eminent a mathematician were to repeat his calculations, assigning a shorter focus to the concave lens; when, I hope, our results would no longer disagree.

If the destruction of the aberrations could not be completed by the methods proposed, the construction of the correcting lens would certainly demand that great care which Signor Santini considers necessary; for it would require, as in the ordinary construction of an achromatic, a very nice adaptation of the foci and curvatures of the lenses to the refractive and dispersive powers of the glass. But the changes of position, by changing the apertures, and, consequently, the aberrations of the lenses of the correcting glass, give us so great a command over these aberrations, that this construction neither requires a very accurate knowledge of the refraction and dispersion of the glass employed, nor a great nicety in the foci and curvatures of the lenses; and, therefore, it is much easier in practice than the common construction.

This I have verified by actual experiment; having had a telescope made upon this principle, calculating from the average values of refraction, assuming a dispersive ratio somewhat lower than is met with, and making the focus of the concave lens superabundantly short. Having, by these means, given to the correcting lens a surplus of concave spherical aberration, and a surplus of dispersive power, I was enabled, by varying the positions of the lenses, to reduce these surplus aberrations, so as to render the performance of the telescope not inferior to that of an achromatic of the ordinary construction. The object lens is of crown glass, 61½ inches focus, and 5 inches aperture; the convex of the correcting lens is also of crown, the radius of each surface 9 inches, and the aperture 3 inches; the concave is of flint glass, the radius of the first surface 9 inches, that of the second surface 10.4 inches; the distance of the correcting lens from the object-glass is 25.1 inches; and the separation of the lenses 0.16 inch. The correcting lens acts as a concave, extending the focus to 64.7 inches. The two convex lenses were originally of plate glass; but they were so veiny and imperfect in other respects, that I had them replaced by two others of crown glass, made by Mr. Dollond, in whose hands the telescope was a short time ago.

The danger of deranging the centering of the lenses, by the minute separation required, does not appear insurmountable by very simple mechanism; two tubes, sliding steadily upon one another by the action of a screw, seem sufficient for the purpose. One advantage, however, might be obtained by giving up this facility of adjustment; for by making the interior surfaces of the correcting lens to coincide, and by cementing them together, we could prevent all loss of light by reflection from these surfaces. Nor does the construction appear, even with this limitation, so difficult as the ordinary one; since the alteration of one surface only, namely, the second of the concave lens, would be necessary. If the curvature of this surface were too great, the surplus of concave aberration would have to be removed by flattening it, instead of separating the lenses.

ALEXANDER ROGERS.

Leith, 28*th October*, 1829.

Chemical Examination of a Native Arseniuret of Manganese.

By Robert John Kane, Member of the Meath Hospital Medical Society, Dublin.

SOMETIME since I obtained from a person, who had a collection of minerals for sale, a number of specimens of various ores of manganese, amongst which was one ticketed "Manganese Ore, Saxony." It was sold me as a specimen of the native peroxide. It was rather a small piece, about two and a half ounces weight, and being based on a mass of foliated galena, was pierced, in every direction, by small veins of ferruginous quartz. I did not take much notice of it at the time, but some months afterwards, being desirous of comparatively examining the different specimens of the native oxide which I possessed, I exposed, in a tube retort, to a red heat, a few grains of this mineral. No oxygen came over, but I was much surprised to see that a substance rose in vapour, and condensed under the form of brilliant acicular crystals on the inside of the cool portion of the tube; I immediately removed the retort from the fire, and in so doing, it cracked. The external air got admission, and the mineral inflamed, burning with a blue flame, throwing off copious white vapours, and emitting a strongly alliaceous odour.

I was surprised at this phenomenon, as there had been no native arseniuret of manganese described by mineralogists, therefore I resolved to more thoroughly investigate the subject; and after the most careful study of the nature of this substance which I was competent to effect, I can reconcile the appearances which I observed, to no other supposition than that which I before mentioned.

EXTERNAL CHARACTERS.

The specific gravity of a homogeneous fragment was found to be 5.55.

Hard, brittle, perpendicular fracture, uneven, fine-grained, brilliant; colour greyish white, growing dull, and becoming covered with a fine blackish powder on exposure to the air; horizontal fracture, dull and mammillary. It is very easily broken in this direction; in fact, the ore seems entirely composed of a series of mammillary laminæ.

Before the blow-pipe, it burns with a bluish flame, and emits a smell similar to that of arsenic, when heated strongly. The greater part of the ore sublimes. Whilst the ore is burning, it throws off white fumes which condense on the cold part of the charcoal, under the form of a white powder. When a platina stand was used, the mineral fused and united with it.

This ore is totally dissolved by nitro-muriatic acid.

When boiled in a large quantity of nitric acid it is entirely dissolved; but when the quantity of acid used is small, the ore is converted into a white powder, soluble in more acid.

When re-agents were applied to a solution of this mineral in nitro-muriatic acid, the following effects were observed:—

Alkalies precipitated it white, which gradually passed to brown.

Carbonated alkalies produced a precipitate; the colour of which was more permanent.

Lime-water and

Solution of acetate of lime produced white precipitates, in a portion of the solution which had been rendered neutral. An excess of acid re-dissolved these precipitates.

Tincture of galls did not affect it.

A solution of ferro-cyanite of potassa tinged the liquor of a light-blue, but did not produce any well-formed precipitate.

Hydrosulphuret of ammonia precipitated it of a dirty yellowish white.

A portion of the ore having been heated in a glass tube, the sublimed metallic crystals were separated for examination.

They dissolved perfectly in nitric acid, and the solution being concentrated to expel the excess of nitric acid, was found to possess the following properties :—

When a current of sulphuretted hydrogen was passed through it, there was produced a yellow precipitate.

When neutralized by potassa, and nitrate of silver added, the well-known brick-red precipitate of arseniate of silver was produced.

With nitrate of copper the neutral solution gave a greenish precipitate ; and with hydrosulphuret of ammonia, one of the yellow colour of orpiment, when rendered slightly acid.

After several preliminary trials I resolved upon employing the following process for analytically determining the relative proportions of the constituents.

The ore was digested in nitric acid until it was entirely converted into arseniate of manganese ; to it was then added an excess of a solution of potash, and the liquors were boiled for some time longer. The arseniate of manganese was thus decomposed, and the protoxide eliminated, rapidly absorbing oxygen, was converted into the brown deutoxide of that metal, which being dried and weighed, the quantity of manganese was calculated from it.

The liquor thus freed from the manganese was accurately neutralized by nitric acid ; then a solution of binacetate of lead was added, as long as any white precipitate was produced. The arseniate of lead, thus formed, was dried, weighed, and the quantity of arsenic deduced from it.

From the result of three different analyses, I was induced to adopt the following numbers as the average:—

Manganese	45·5
Arsenic	51·8
Loss and a trace of iron	2·7
	100·0

But this is somewhat an approximation to the numbers of manganese and of arsenic combined atom to atom; and perhaps the difference depends rather on my unskilfulness than on the real composition of the mineral. The relative equivalents might be so arranged, viz.—

	Atomic Weight.	Theory.	Experiment.
Manganese	28	42·4	45·5
Arsenic	38	57·6	51·8
			Loss 2·7
$M^{ng} + A^{rs}$	66	100·0	100·0

A Plan for Improving the Carriage Pavement of the Metropolis. By Lieutenant J. H. Brown, Royal Navy.

HAVING observed the unsafe and disgraceful state of the pavement of most of our public streets, and, even where Macadamizing has been adopted, the repairs continually required, and the constant accumulation of mud and dust, I have given the subject much attention for the last few years, and I am induced to propose a plan to remedy the defect.

I must premise by observing, that the foundations of most of our streets are, of necessity, bad; as, independent of the nature of the soil, the ground has been so often dug up and turned over, for the purpose of levelling, forming and repairing sewers, laying down gas, water-pipes, &c. &c.; that although the paving should be executed in the best manner, it is impossible that it can remain in the position placed; for the water, after a glut of rain, passes down between the joints of the stones, the foundation, composed of earth, rubbish, &c. speedily becomes a puddle, *softer* in some *parts* than in *others*, a great portion of which works and churns up between the joints, and allows the stones to sink under the first heavy carriage that passes, forming pools for the reception of more rain, and daily becoming worse, until it is necessary to relay it; when, after a short time, the same thing happens over again, thus causing an endless expense to the different parishes.

The plan I suggest is, that, after the foundation has been formed in the necessary shape, and the surface rolled or rammed hard, the paving stones, dressed so as to fit close together, should be laid or set in a *thick coat of good mortar*, and the joints grouted with cement; the *whole mass* would thus become a *solid body*, and the rain would be effectually prevented from penetrating to the foundation, which would remain dry and firm in the position it was originally placed.

I recommend this in preference to any other artificial foundation, such as broken stones, sand, gravel, &c.; for, provided it was sufficiently firm to bear the weight of the paving stones and carriages passing, nothing would displace it until worn out.

The stones must be grooved on the surface about a quarter of an inch deep, to prevent horses slipping; and if scored diagonally, it would obviate any jar or jolt that could be anticipated from the wheels of a carriage passing over them at right angles. This scoring could be renewed when necessary, by providing light screens on wheels, inside of which the men could work on any part with hammer and chisel, protected from danger. Carriages would run over this pavement with far less friction, and as little noise, as they now do over a Macadamized road; and this great advantage would be gained, that, whereas the one is constantly wearing in holes and wanting repairs, never looks finished, and covers the inhabitants and travellers with mud and dust, (the only change) a pavement laid down on this principle would last for many years without further expense or trouble; and as no mud could work up from the foundation, consequently no dirt could accumulate, but what fell from the horses; and the small, but gradual and even wearing away of the surface, it could be kept by sweeping as clean as a barn-floor; the expense of watering in summer would also be saved.

This plan would be attended with but little more cost than the old mode of paving, the difference being the dressing the stones, and substituting *mortar* for *gravel;* certainly, considerably cheaper than the method adopted in many parts, of laying a foundation of Macadamized stones. And to prevent the necessity of ever disturbing it for any purpose after it was once made, I would construct culverts * along the sides of the streets, under the part now occupied by the surface-gutters, large enough to contain the branch water-pipes, gas-pipes, or, indeed, anything that could be conveyed through pipes in or out of the houses. The tops of those culverts should be of cast iron, about four feet long, and each length, if necessary, (or wherever a joint or communication was required) should *lift on or off* at pleasure. The inspector or workmen could thus, at all times, have easy access to the pipes, without interfering with the street or house; and any accident could be amended cheaply and promptly, (the men having day-light and room to work) without the expense and annoyance of breaking up the pavement, which could never be properly replaced. The upper surface of those cast-iron lengths should be hollowed into the form of a gutter, in order to carry the rain water from the street to the grating which communicates with the sewer; and the bottom of the culvert would convey any water that leaked through the joints, in the same manner.

For a new pavement, I would recommend the best material (granite) to be used, as the cheapest in the end; but in relaying a street, the same stones may be used, after being dressed or cut to the necessary form. I consider the best proportions to be eight inches long, five inches wide; and a foot, or as much more as convenient, deep. If it was necessary to use different sizes, they must on no account be mixed, but worked into separate lengths or patches of pavement.

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* The mains as they are at present constructed, may run through the centre of the street; as they would seldom or ever want to be disturbed, and they could be inspected when the pavement was worn out, and in the progress of relaying; or they could run along the sides, if the culverts were made large enough; of course the size of the side culverts or gutters, depends on the situation and the quantity of water required to be conveyed.

Although Macadamizing is undoubtedly a great improvement on our roads, it can never come in competition with this mode of paving for streets, or great thoroughfares, which has all the advantages of *strength, cheapness, beauty, durability, and cleanliness.*—June 25th, 1829.

Since making the above observations, I have noticed that this method has been *partly* adopted in different streets in towns; that is, so far as dressing the stones and grouting the joints, a considerable step towards improvement; *but this of itself is not sufficient,* for if a stone rests on earth, gravel, or broken stone, for the bed, it is liable to be disturbed by the shock of a heavy carriage passing, in which case the grouting in the joint will crack, and admit the wet to the foundation, causing all the mischief I have already described. Another very bad practice I have observed, is that of ramming the stones, to bring the pavement to a level, some days after it has been grouted, and in reality destroying the benefits to be derived from it, as good mortar or grouting should set and get hard, in a very short time after it is first applied, and the ramming cracks the joints again: besides, all the foundations I have seen are *porous,* and although being for the most part of broken stones, they will not work or churn up, as the gravel does, (which accounts for the mud we wonder to see in the streets that are so often swept), still, they allow the *water* to *pass* through them to the *real* foundation below, causing the pavement to sink into inequalities, as may be now seen in Fleet-street, which was done at a great expense about two years ago.

By *bedding* the stone in mortar, properly *placed in the situation it is to remain,* then grouting the joint, and allowing it to set hard, without *afterwards ramming or disturbing it,* the pavement will remain *immoveable* and *water tight,* until fairly worn out, and save all the expense of an artificial foundation of Macadamized stones, or other matter. Now, when we consider the labour of digging out, and carting away, eighteen inches or two feet deep of earth, and replacing it with another material to be provided, prepared, and carted also; such expense, which I maintain is all *unnecessary*, and which must make the *principal item* in the cost of payments I have lately seen, may all be saved by this plan.

A grand objection to a Macadamized pavement, in this and every cold climate, is, that a severe frost setting in after wet, does *incalculable* injury, owing to its porous state; now, as no water can penetrate *beneath the surface* of this pavement, if properly made, this serious fault is obviated.

I trust the strength and cheapness of this plan, as compared with all others I have seen, will induce some one, having the power, to grant it a trial, and I have no fear for the result. I have observed that the most useful inventions are the most simple and obvious when once made known; and often, from their very simplicity, take away from the merit of the inventor; we may instance steam, gas, &c. &c. As I publish my sentiments and observations, and the result I have come to, after much trouble and attention, without any sinister motive, I trust I may escape the imputation of having promulgated a plan which any one could suggest; should such casuists be found, I refer them to the story of Columbus breaking the egg.

September, 1829.

On some Pharmaceutical Preparations of Iron, and particularly the Tartrates. By Andrew Ure, M.D., F.R.S., &c. &c.

1. Tartaric acid has hardly any action on the red oxide of iron, for though 200 grains of the former dissolved in water, were digested on a sand bath on 50 grains of the latter, in the form of the rubigo ferri of the shops, it became but faintly coloured in the course of three days, and a very few grains only of the oxide were taken up. The same rust of iron was quite soluble in dilute muriatic acid.

When tartaric acid, in solution, is digested on red oxide of iron, prepared by nitric acid, no apparent combination ensues after many hours, and the re-crystallized acid is nearly colourless.

2. The readiest mode of obtaining a proper red tartrate of iron, is, by mixing the liquid red sulphate with solution of tartrate of potash in equivalent proportions. Sulphate of potash precipitates in a crystalline powder, (the solutions being somewhat concentrated) which may be separated from the blood-red liquid tartrate of iron by filtration. When to this ferreous solution, its own bulk of alcohol, sp. gr. 0.840 is added, so as to form a proof spirit menstruum, decomposition immediately ensues, indicated by a cloudiness, and a precipitate of a treacly consistence and aspect, which collects at the bottom. The supernatant liquid is nearly colourless, and contains hardly any iron, but much tartaric acid. The viscid precipitate soon hardens into a brittle mass of subtartrate of iron, insoluble in water. Thus it appears, that a spirituous menstruum is not at all adapted for holding red tartrate of iron in solution, though Madeira and Teneriffe wines of common strength answer very well.

When the above concrete precipitate is treated with water, acidulated with tartaric acid, it readily dissolves, with the reproduction of red tartrate of iron.

The subtartrate, when newly thrown down, is fusible, at the heat of 180° or 190° F. It burns reluctantly in the flame of a spirit-lamp, with a faint ignition, and a slight smell of caromel.

3. The potash-tartrate of iron, as prepared by the process of the London Pharmacopœia, is a powder of an olive-green colour, occasionally tinged with brown. When 100 grains of it were heated to the temperature of 160° F., they lost 4 grains; but this loss will vary according to the manner of preparing it. By dull ignition, in a platinum crucible, it emits a lambent blue flame, and is converted into red oxide of iron and carbonate of potash, amounting together to 52 grains. The alkali was dissolved out with water, and tested with acid. It indicated 18 grains of potash, equivalent to 46.5 of tartrate of potash. The

peroxide of iron weighed 32 grains. This existed in the original compound, partly as a tartrate, and partly as a subtartrate; for not more than two-thirds of the original powder are soluble in water.

If one volume of the solution of the potash-tartrate (in about seven times its weight of water) be mixed with one volume of alcohol, sp. gr. 0.840, so as to form a proof-spirit menstruum, the subtartrate of red oxide of iron immediately forms, and falls in a viscid mass, and the spirituous liquid becomes nearly colourless, containing very little iron.

Though the spirituous vehicle, prescribed in the Pharmacopœia, be weaker than proof, there can be no doubt from the above experiments, that a dilute alcohol is not nearly so proper a menstruum for this triple salt of iron as Madeira wine, which, containing a considerable portion of acid, will form a more powerful and permanent solution.

4. Medical men have, in modern times, probably paid too little attention to the state of oxidizement in which they administer iron. The older chemical physicians of the celebrated school of Stahl, taught, and I believe justly, that, according as this metal is differently prepared, it acquires powers over the body of a different, and almost opposite, nature. Some preparations were said to promote the motion of the fluids through the whole system; while others repressed or obstructed these motions. The remarkable stimulant and deobstruent virtue displayed by iron in the cure of chlorosis, was, at that period, attributed to one of its supposed constituents, the *phlogiston;* as the astringent property was referred to the *earthy* ingredient. When these notions, derived from "old experience," are expressed in modern phraseology, we may say, that the mildly exciting power of iron will be found in its metallic or protoxide state; while its acrid and constringing qualities may be sought for in its peroxide, and in certain saline compounds, where the acid contributes its share of the effect, as is the case with the sulphate.

In fact, it may be affirmed, that iron, like copper and mercury, acquires acrimony (pathologically speaking) by per-oxidizement; a conclusion which would have been more generally drawn, had a good form of protoxide preparation existed in our Pharmacopœias, or our shops. The *precipitated subcarbonate of iron* is merely the peroxide associated with only from 3 to 5 per cent. of carbonic acid; and is, therefore, not entitled to its pharmaceutical name.

A very pure, mild, and permanent form of a protoxide-salt may, however, be easily obtained by exposing clean particles of iron, as bits of iron wire, to the action of tartaric acid and water at a gentle heat. An effervescence ensues, hydrogen is disengaged from the water, the iron is oxidized to a *minimum*, and is fixed in that state by its instantaneous combination with the acid of tartar. This tartrate owes its permanence to its insolubility; but yet (like iron filings and calomel) it acts energetically on the system. The proto-tartrate of iron is nearly white, and pulverulent. The powdery matter, as diffused in the liquid, may be decanted off the iron into a filter, and washed with a little water. It has a mild chalybeate taste, and will constitute a valuable accession to the *Materia Medica*.

At a dull red heat, this tartrate readily takes fire, and burns slowly away like tinder, after its removal from the source of heat, with the exhalation of a caromel odour; while the oxide of iron becomes peroxidized.

Glasgow, December 5, 1829.

MISCELLANEOUS INTELLIGENCE.

§ I. Mechanical Science.

1. *On Measuring the Force of Pressure, by* Mr. Bevan.—Mr. Bevan describes his highly useful and applicable method in the following manner:—" If we take a leaden bullet of any determinate diameter, and expose it to pressure between plates of harder metal, made to approach each other in parallel position, the bullet will be compressed or flattened on two opposite sides in an equal degree: provided the lead is pure the degree of compression will indicate the amount of pressure. With a graduated press of the lever kind it will be easy to form a scale of pressure corresponding to the different degrees of compression, until the ball is reduced to a flat circular plate of about one-fifth of an inch in thickness; and it will be found, that an ordinary bullet, of about five-eighths of an inch in diameter, will require a pressure of near 4000 pounds to effect this degree of flattening. Suppose, therefore, we wish to measure an actual pressure supposed to be nearly 20 tons, we have only occasion to place 10 or 12 of these balls at a proper distance asunder, so as not to be in contact when expanded, and then to measure, by good callipers, or other suitable means, the compression of each ball either by its thickness or diameter, and afterwards add into one sum the particular pressure due to each ball, from the scale first made, by using the lever press before mentioned."

By this mode I have ascertained the amount of friction of an iron screw press, with rectangular threads, to be from three-fourths to four-fifths of the power applied; or the actual pressure has not exceeded 4 or 5 tons; when the calculated pressure, if there had been no friction, would have been 20 tons.

The larger the ball the greater will be the pressure necessary to reduce it to a given thickness. An ordinary leaden shot of one-eighth of an inch diameter will require nearly 100 pounds to compress it into a flat plate. By using a ball five-eighths of an inch diameter, I have found the actual pressure of the common bench vice to be about 2 tons; when under the same force, if there had been no friction, the pressure would have been 8 tons.

In the practical application of these balls it will be convenient to make a small impression upon them with a hammer before they are placed between the plates, to prevent them from rolling out of their proper position: this operation will not be found to interfere with the result, as it is the ultimate compression only that is sought, and which is not affected by that of a smaller degree before impressed. Hence the same substance may be used several times, provided the succeeding pressure exceeds that of the preceding.

It may be observed, that the application of these leaden balls to determine the actual pressure, will not interfere with the regular operation of a press, as the articles under pressure may be in the press at the same time the balls are used, which of course must be placed between separate plates.—*Phil. Mag.* N. S. vi. 284.

2. *Night Telegraph.*—A night telegraph, invented by M. Lecot de Kerveguen, appears from the French journals to attract some attention, because of its novelty, cheapness, and applicability, both by day and night. The inventor has gradually improved it until he can obtain 29,245 signs by its means. It consists of a cabin with two faces, for the purpose of transmitting signals in two directions. These faces are pierced with three circular holes, each divided by a vertical and a horizontal diameter. Each hole is covered by an opaque black disc, in which is opened a ray or line, which is rendered white for day observation, and luminous at night time. Motion is given to the discs, and consequently to the rays from within the cabin, so that right or acute angles to the right or the left, upwards or downwards, may be made at pleasure. The dimensions of the cabin are proportional to the diameter of the discs, and the rays on these are themselves proportional to the distance between the telegraph and its correspondent one.

The following are some of many experiments made with this apparatus: they took place on the 21st of March, at eight o'clock in the evening by bright moonlight:—

The first ray was	4 feet 6 inches long by	8 inches wide.
second	4 do.	6 do.
third	3 do.	4 do.
fourth	2 do.	3 do.

The telegraph was at the port of Toulon, and all the signals were well observed by the men at Cape Sepet, which is 1⅘ leagues distant. On the following morning the experiments were repeated with the white rays by day-light. From these experiments it appeared that the smallest ray was abundantly visible at the distance mentioned, and experiments since have shewn at a distance even more than double.

Still more recently, M. Kerveguen has so far simplified his arrangements as to use only one ray or bar of light; and though he has by doing so reduced his arbitrary signs to 8649, yet that is a number abundantly sufficient for all ordinary uses. In the new experiments made the smallest ray was used, and still found fully sufficient for its intended purpose.—*Revue Ency.* xliii. 763.

3. *Chinese Canal.*—A canal was opened in 1825, to the west of Sargan, in Cochin China, which connected that town with a branch of the river Cambodja. Its length was 23 miles, its width 80 feet, and its depth 12 feet. This canal was begun and finished in six weeks, although it had to be carried through large forests and over

extensive marshes: 20,000 men were at work upon it day and night, and it is said that 7000 died of fatigue. The sides of the canal were soon covered with palm-trees, for the cultivation of which the Chinese pursue a particular method.—*Allg. Handl. Zeitung*, 1825.

4. *Method of removing fixed Glass Stoppers.*—M. Clausen recommends that in these cases a piece of woollen list should be passed once round the neck of the bottle, and the two ends taken by two different persons; then the bottle being held firm, if the persons draw the list alternately towards them, the friction upon the neck of the bottle will soon warm it so much as to enlarge the glass, and allow the ready removal of the stopper. In place of this M. Chevalier recommends the old process of warming the neck either by a hot coal or a flame. He adds, if the top of the stopper is broken off so that no hold of it can be taken, the bottle should, after being warmed, be enveloped in a cloth, so as to leave the neck free, and then be struck upon the bottom (by the hand). Generally the first blow will make the stopper fly out; but sometimes several successive blows are required to effect the separation.

5. *On the Causes of Diffraction*, by M. Haldat.—The phenomena of diffraction, the examination of which, in latter times, has furnished such powerful arguments against the hypotheses of Newton, and drawn philosophers towards the opinion of Descartes, have appeared to M. Haldat as if they had hardly been sufficiently discussed in relation to the circumstances which might modify or elucidate their cause. It is with this in view that he has undertaken numerous experiments, in which the bodies producing diffraction, which he calls diffringent bodies, have been submitted to the action of agents the most proper to modify them, and as an attractive force is the power by which the Newtonians explain diffraction, he used in his trials all those agents most likely to affect it.

After assuring himself, as had been announced by other experimenters, that this phenomenon was not modified either by the specific gravity or the chemical nature of the body, M. Haldat turned his attention to the strongest natural powers; heat, electricity, magnetism, electro-chemical currents, and finally, affinity, so powerful in modifying the attractive force, have been successively and simultaneously employed to modify the state of the body, whilst it exerted that influence upon the luminous rays which occasions diffraction, without, however, producing any sensible alteration in the phenomena. Thus metallic wires and diffringent plates of iron, copper, and silver, have been heated to whiteness, and cooled to 14° F.; and yet the coloured bands produced by their action upon the rays of light have not presented an appreciable difference from those produced by the same bodies at common temperatures.

Diffringent wires and plates have been made the channels for currents of ordinary electricity for violent discharges of powerful batteries, and for electro-chemical currents sufficiently powerful to ignite and fuse them. Currents moving in the same and in opposite directions have been used. The ray of light has been received on the edges of diffringent plates connected with powerful magnets, yet without any appreciable alteration. The rays of light, before arriving at the diffringent body, have themselves been traversed by powerful flames, by strong electrical currents and discharges, without any change being perceived in the fringes and other appearances of diffraction. The obscure bands in the shade of these wires have remained invariable both in intensity and dimension.

From these experiments, M. Haldat thinks, that the explanation of diffraction, founded on an attractive force, or on certain atmospheres supposed to exist around the body, can hardly obtain the assent of philosophers, when this attractive force and these atmospheres being submitted to agents so fitted to alter them, have produced no change in the phenomena. These facts certainly do not establish the theory of undulations, but they help to destroy the only explanation which can be opposed to it. The author, however, does not hide the difficulties which these experiments also present to the theory of undulations, and inquires how it is possible that the motion of the luminous waves, which ought to be so regular and constant, is not disturbed by the flowing out of subtile fluids which strike against them in their course. He refers the solution of this question to the period when science shall have penetrated more intimately into the nature of those agents which at present are known to us only by their effects.—*Ann. de Chimie*, xli. 424.

6. *On the Impressions produced by Light on the Eye.*—The following are the conclusions to a mémoire on this subject by M. Plateau. *First section*—i. Any sensation of light whatever requires an appreciable time for its complete formation, and also the same time for its complete disappearance. ii. The sensations do not disappear suddenly, but gradually diminish in intensity. iii. As a sensation fades, the progress of its decrease is slower as the effect is nearer to a close. iv. Different colours illuminated by daylight produce sensations differing little from each other in their total duration. Their order, in this respect, beginning with that which produces the longest sensation, is white, yellow, red, blue. v. The total duration from the time when the sensation has acquired its fullest power, to that when it is hardly sensible, is very nearly 0.″34. vi. Finally, it results accidentally from the experiments, that the principal colours arranged according to the intensity of the sensations which they are competent to produce, stand in the following order, white, yellow, red, blue.

Second Section—i. New proofs confirm the order of colours contained in the sixth result of the first section. ii. The visual angles under which M. Plateau can see the different colours, are as follows:—

	In the shade.	In sun-light.
White	18″	12″
Yellow	19″	13″
Red	31″	23″
Blue	42″	26″

The angles observed in sun-light are nearly a third of those in the shade. iii. When the sensations of two different colours succeed each other on the retina with a velocity less than that necessary to make the two impressions appear as one, there generally appear certain shades which are extraneous to the two colours employed, or to their mixture; by this means a fine white can be obtained when the yellow and blue colours only are used. iv. When two alternating sensations succeed each other with such rapidity that they produce but one impression, the latter does not always present a colour which would result from the mixture of the former; thus, combining the effect of yellow with that of deep blue in the way just mentioned, a grey colour can be produced without the least appearance of green. v. With the exception, perhaps, of yellow, the sensations of certain colours do not act in their combination with other sensations in the order of the intensity of their colours; their maximum of influence exists in a certain pale tint, on each side of which their influence diminishes: thus, the blue colour of maximum power with respect to red and yellow, is that of the sky in its most coloured state.—*Bull. Univ.* A. xii. 123.

7. *Brewster's Monochromatic Lamp.*—Dr. Brewster makes a monochromatic lamp by taking a portable gas lamp and fixing a platina wire, or a plate of mica in the flame; platina wire spirals answer very well until they become covered with carbon. A large cotton wick also, imbued with sal ammoniac, produces a good effect in a hot gas flame. The supply of solution of muriate of ammonia, required to continue the light, may be given by means of a large sponge, or a capillary fountain. Dr. Brewster has used these monochromatic lamps very much in microscopical researches.—*Edin. Jour.*

8. *Artificial Magnets and their uses.*—Extract of a letter from Strasbourg:—Dr. Keil, of Langensalza, has discovered a method of making artificial magnets of enormous force, and infinitely more powerful than those as yet made, without great increase of dimensions. He has also, by their means, cured many diseases dependant upon the nervous system. The most powerful magnets as yet made, have been large, and not supported more than 88 or 90 pounds; but M. Keil has made one which sustains 45 pounds, and yet weighs itself only 3 pounds 6 ounces; he has another which

supports 480 pounds. This magnet is in the form of a horse-shoe, and is composed of 9 plates; its length is 17 inches, and its weight 43 pounds.

I have myself seen (says the writer) this magnet carry the enormous weight mentioned, and M. Keil says he can make them still stronger, and competent to carry five times this weight, without increasing their size in a greater proportion. He can also strengthen old magnets, but never bring them near to those manufactured upon the new plan.

This discovery is said to be of great importance in the curative art. The magnetic influence upon diseases has been supposed to have been long known, and even cures affected in some cases by making passages over the affected part by means of magnets. But the bars used were too feeble; and it appears that those belonging to M. Keil which produce singular effects, are alone capable of demonstrating the power of mineral magnetism over diseases. He has succeeded in very violent rheumatic pains, epilepsy when not due to organic lesions, cramp in the stomach, feebleness of sight, goitres, head-ache, tooth-ache, tic douloureux, &c. The writer's brother was a witness of one cure effected in a case of rheumatism.

M. Keil also proposes to publish his investigations of terrestrial magnetism, and the theory of magnetism, in which it is said he has much new matter. He is on his way to Paris, so that we may hope very soon to know the truth of the various matters stated, which, if they have any reasonable foundation, cannot fail to be of great importance.—*Bull. Univ.* A. xii. 241.

9. *Application of Magnetism to Medicine.*—Dr. Becker has also published a long account of effects produced by powerful magnets on nervous and other patients. His conclusions are, that, i. Mineral magnetism is a very efficacious agent in nervous pains, especially when they are of long standing; ii. That it is not of any utility, but rather injurious, when inflammation or any other excitation of the vascular system accompanies these pains; iii. That its effect is less sure on recent nervous cases, because they are frequently accompanied by unperceived febrile affections.—*Hufeland's Journal.*

10. *Daily Magnetic Variation.*—According to experiments made by Humboldt, it appears that the daily variation is by no means the same in different places. On January 29th, the variation at Berlin was three times greater than on the 27th; whereas, at Paris on the 29th, it was much less than that of the 27th at Berlin. At Berlin, the variation, on the 11th of January, was twice that of the 10th at Paris; that of the 10th was greater than that of the 11th. These results do not appear to depend on any error of observation, but are real consequences of local causes. By trials made at the mouth and lowest part of the Freyburg mines, it was found that a depth

of 798 feet in the earth had no sensible influence upon the inclination of the needle.

11. *Use of Muriatic Acid in cleansing Monuments.*—The time and expense necessary in cleansing stone buildings and monuments by scraping them, induced M. Chevalier to try other methods. He finds it may be done, i. by dry brushing, but the process has inconvenience; ii. by washing first with a brush and water, then with water acidified with muriatic acid, and finally by water alone; iii. by washing with a weak alkaline solution, and then with a very dilute acid. The second process is considered as the most advantageous, and has been applied on the walls of the Ecole de Médecine. It is now under trial upon the marble statues of the gardens and halls in the building.

12. *Preservation of Firemen exposed to Flames.*—The Chevalier Aldini of Milan has been earnestly occupied in the construction of an apparatus, or rather clothing, intended to preserve persons from injury who are exposed to flames. The apparatus has lately been fully tried at Geneva, and an account of it, and the trials, given in the Bibliothèque Universelle. A union of the powers possessed by a metallic tissue to intercept flame, with the incombustible and badly conducting properties of amianthus, or other substances, has been made in the apparatus; and the latter consists of two distinct systems of clothing, the one near the body composed of the badly conducting incombustible matter, and the other, or external envelope, of a metallic tissue.

The pieces of clothing for the body, arms, and legs, are made of strong cloth which has been soaked in a solution of alum; those for the head, the hands, and the feet, of cloth of asbestos. That for the head is a large cap, which entirely covers the whole to the neck, and has apertures in it for the eyes, nose, and mouth, these being guarded by a very fine copper-wire gauze. The stockings and cap are single, but the gloves are double, for the purpose of giving power of handling inflamed or incandescent bodies.

M. Aldini has, by perseverance, been able to spin and weave asbestos without previously mixing it with other fibrous substances: the action of steam is essential in the bending and twisting of it, otherwise the fibres break. The cloths prepared with it were not of close texture, but loose: the threads were about one-fiftieth of an inch in diameter, and of considerable strength: cords of any size or strength may be prepared from them. M. Aldini hopes to be able so to prepare other fibrous matters, as to be able to dispense altogether with this rare and costly material.

The metallic defence consists of five principal pieces: a casque, or cap complete, with a mask: this is of such size as to allow of sufficient space between it and the asbestos cap, and is guarded before the face by a visor, so that the protection is doubled in that part; a cuirass, with its brassets; a piece of armour for the waist and thighs; a pair of boots of double wire-gauze; and an oval shield, five feet long, and two and a half wide, formed by extending gauze over a thin frame of iron. The metallic gauze is of iron, and the intervals between the threads about one-twenty-fifth of an inch each.

When at Geneva, M. Aldini instructed the firemen in the defensive power of his arrangements, and then practised them, before he made the public experiments. He shewed them that a finger enveloped first in asbestos, and then in a double case of wire gauze, might be held in the flame of a spirit-lamp or candle for a long time, before inconvenient heat was felt; and then clothing them, gradually accustomed them to the fiercest flames.

The following are some of the public trials made. A fireman having his hand inclosed in a double asbestos glove, and guarded in the palm by a piece of asbestos cloth, laid hold of a large piece of red hot iron, carried it slowly to the distance of 150 feet, then set straw on fire by it, and immediately brought it back to the furnace. The hand was not at all injured in the experiment.

The second experiment related to the defence of the head, the eyes, and the lungs. The fireman put on only the asbestos and wire gauze cap, and the cuirass, and held the shield before his breast. A fire of shavings was then lighted, and sustained in a very large raised chaffing-dish, and the fireman approaching it, plunged his head into the middle of the flames, with his face towards the fuel, and in that way went several times round the chaffing-dish, and for a period above a minute in duration. The experiment was made several times, and those who made it said they suffered no oppression or inconvenience in the act of respiration.

The third experiment was with the complete apparatus. Two rows of faggots, mingled with straw, were arranged vertically against bars of iron, so as to form a passage between, thirty feet long, and six feet wide. Four such arrangements were made, differing in the proportion of wood and straw, and one was with straw alone. Fire was then applied to one of these double piles; and a fireman, invested in the defensive clothing, and guarded by the shield, entered between the double hedge of flames, and traversed the alley several times. The flames rose ten feet in height, and joined over his head. Each passage was made slowly, and occupied from twelve to fifteen seconds; they were repeated six or eight times, and even oftener, in succession, and the firemen were exposed to the almost constant action of the flames for the period of a minute and a half, or two minutes, and even more.

When the course was made between the double range of faggots without straw, the fireman carried a kind of pannier on his back, prepared in such a way as to be fire-proof, in which was placed a child, with its head covered by an asbestos bonnet, and additionally protected by the wire-gauze shield.

Four firemen made these experiments, and they agreed in saying, that they felt no difficulty in respiring. A very abundant perspiration came on in consequence of the high temperature to which they had been exposed, but no lesion of the skin took place except in one instance, where the man had neglected to secure his neck by fastening the asbestos mask to the body dress.

No one present could resist the striking evidence of defence afforded when they saw the armed man traversing the undulating flames, frequently hidden altogether from view by them as they gathered around him.

The fact that in M. Aldini's apparatus a man may respire in the middle of the flames is very remarkable. It has often been proved, by anatomical examination, that in cases of fire many persons have died altogether from lesions of the organs of respiration. It would appear that the triple metallic tissue takes so much of the caloric from the air as it passes to the lungs, as to render its temperature supportable; and it is known, by experiments in furnaces, that a man can respire air at 120 or 130° C. and even higher. Perhaps also the lesions referred to may have been due to aqueous vapour, which is often produced in great abundance in fires where endeavours are made to extinguish them by water, for such vapour would transfer far more heat to the lungs than mere air. Hence in every case, and however guarded, firemen should enter houses in flames with great prudence, because the circumstances are not the same as in the experiments just described.

It is remarked that several suits of this defensive clothing should be provided, not to clothe many persons at once, but that, in endeavouring to save persons or valuable things in cases of fire, the fireman should not approach again and again in heated clothing, but have a change at hand. The Grand Duke of Tuscany has ordered six suits for the city of Florence.

M. Aldini shewed several experiments relative to the extinguishing power of his preparations before the Société de Physique de Genève. One consisted in placing an asbestos cloth of loose texture over a flame either of wax or alcohol; the flame was intercepted as well as it could have been by a piece of wire gauze. This experiment is supposed to favour the objections made to Sir H. Davy's explication of the theory of the wire gauze safety-lamp; but there seems to be a mistake in the idea which has been taken of that theory. Sir H. Davy never explained the effect of his lamp by absorption of heat from flame dependant upon the good conducting power of the tissue alone, but by the joint action of absorption and radiation. There is no doubt that cloth of asbestos is an admirable radiator, and that this power, with its conduction, is probably sufficient to explain the effects upon Sir H. Davy's theory.—xli. p. 333.

13. *Astronomy.*—It was mentioned in a former number of this

Journal, that a plan for a minute survey of the heavens had been proposed by, and partially executed, under the superintendence of the Royal Academy of Sciences of Berlin. The nature of this plan we previously detailed; and stated at the time, that two maps, one by M. Inghirami of Florence, the other by M. Harding of Gottingen, executed in accordance with it, had been delivered to the Academy. Since then, a third map has been contributed by a countryman of our own, the Rev. T. Hussey, with the assistance and co-operation of the Rev. F. Dawson. Relative to this work, which required three years for its completion, the following letter, addressed to the former of these gentlemen, by M. Encke, secretary to the Royal Academy of Sciences of Berlin, has been communicated to the Astronomical Society of London:—

"You have doubtless, Sir, been surprised at not receiving any earlier information of the arrival of the map and catalogue of stars which you delivered to the Prussian ambassador in July last. You will excuse me, however, on my assuring you, that the parcel did not reach my hands till the 22d of October. This great delay has, fortunately, not occasioned injury to the drawing you so zealously executed. The whole was in very good condition. It is at present at Konigsberg, whither I sent it immediately after having compared the sixty squares, which are common to yourself and M. Harding, from whom we have been so fortunate as to receive the XVth hour.

"The agreement of the observed stars is almost perfect. As to those which are added, it appears, that the faintness of their light has not permitted so exact an accordance. Sometimes M. Harding has thought right to insert a few points which are not to be found in your map, sometimes the latter contains a few more than M. Harding's map. I hope it will soon be in my power to inform you of the opinion of my colleagues, both as to this map and to your obliging offer, to undertake another part of the heavens, if that should be necessary for the completion of the work—but I am unable to anticipate the decision of the committee. I would not longer delay removing your anxiety as to whether a work on which you had bestowed so much valuable time had reached us safely. Requesting you to accept the assurance of my high esteem,

"I have the honour, Sir, &c.

Berlin, November 4, 1829. "Encke."

When the variable nature of this climate is considered, and the particularly unfavourable state of the weather during these last two seasons, when the XIVth hour was above the horizon, we are by no means surprised, that a discrepancy should exist in inserting in a map stars below the ninth magnitude, when, moreover, those 15° south of the equator, did not attain at Chislehurst, where the comparison between the map and the heavens was instituted, a sufficient elevation to be free from the vapours of the horizon itself.

14. *Achromatic Spectacles.*—In our last number we mentioned a very ingenious artificial horizon, executed by Mr. Newman, of Regent-Street, and passed a well-merited eulogy upon his instruments in general. We have recently been called upon to examine some achromatic spectacles, which he has constructed especially for the use of the medical department in the West Indies. For persons requiring glasses of from four down to one inch focal length, and such are all who have undergone the operation of couching, these achromatic spectacles are invaluable: not only is *all* false light gotten rid of, but the extent of field that is gained would not be credited, except by those who have tried them. Where lenses of greater focal length than four inches are employed, the resulting advantage is but trifling. We feel pleasure in noticing this improvement made by a respectable and deserving artist.

15. *Economical process for imitating Silver Paper.*—The following Chinese process has been made known in Europe by Père Du Halde. Take two scruples of gelatine, or Flanders glue, made of ox hide, one scruple of alum, a pint of water. Place the whole over a slow fire until the water is almost entirely evaporated; spread some sheets of paper upon a table, and with a brush lay on two or three coats of this glue; then take a powder made of a certain quantity of talc boiled, and one-third of the same quantity of alum. After having well pounded these substances, sift them, then boil them again in water, then dry them in the sun and pound them again. The powder, which is then very fine, is to be passed through a very fine sieve upon the sheets of prepared paper. The talc powder is glued fast; it is then to be dried in the shade; after which, remove the superfluous powder with a piece of cotton.—*Journal des Connaiss. Usuelles.*

16. *On the Magnetic Influence of the Violet Ray, by* Professor Zantedeschi, of Pavia.—When Professor Morichini published in 1812 his experiments on the magnetic influence of the violet ray, there was no natural philosopher in Europe who did not wish to repeat and vary them; but, unluckily, the attempts of the most skilful men were not always attended by the success they had a right to expect. Thus it is not surprising that many of the most eminent savans doubted the results of the Italian professor. It was only in 1826 that Mrs. Somerville confirmed, by the most decisive experiments, the fact which he had advanced, that the violet ray possessed a magnetic property. Nevertheless, philosophers were not, as yet, satisfied; they could not verify when they pleased the results obtained, nor discover the causes which impeded the success of their experiments. This state of things led me to undertake a series of experiments on the subject, in this very town, where Professor Configliacchi, in the year 1813, had already made some remarkable attempts. It was only with great distrust that I under-

took anew, a work which this savant appeared to me to have completed with admirable sagacity; nevertheless, the success of my researches has exceeded what I could have hoped. I shall briefly state the method which I followed, and the causes which may prevent the magnetization which it is the object to produce.

As for the method, I introduce into a dark room a solar ray, by means of an heliostat, and decompose it, so that the spectrum is formed horizontally; I then place under the violet ray, in a direction perpendicular to that of the magnetic meridian, the extremity only of the wire that I wish to magnetize. In this way I obtained the following results:—

i. Having placed in the above position a wire of soft iron highly polished, four inches in length, and a quarter of a line in diameter, at the end of 5 minutes, I found that the extremity exposed to the violet ray had acquired a north pole. At the end of 8 minutes, this wire, presented to a magnetised needle, shewed very decidedly that it had poles.

ii. I exposed, in the same way, two wires of soft iron, similar to the preceding, to the action of white light; at the end of 5 minutes each of the two exposed extremities had acquired a north pole; but it was weak, and at the end of a few minutes had disappeared. In the first case, as in the present one, I ascertained, carefully, that the wires employed did not previously possess any sensible magnetism.

iii. The violet ray reversed the very decided poles of a wire of soft iron; it developed, very distinctly, in 6 or 7 minutes, those of another wire, which evinced at its two extremities a very feeble repulsion for the pole of a needle.

iv. Having placed one extremity of a magnetized needle in the red, orange, yellow and green rays, and having observed the nature of its poles and their energy, at the end of 6 or 7 minutes I found no alteration whatever; neither did I remark any effect produced by this operation on a needle which had no perceptible magnetism.

v. A soft iron wire, covered with a coat of rust, and strongly magnetized, having been exposed to the violet ray, in 3 minutes the south pole was converted into a north pole.

vi. A wire of soft iron, highly polished and magnetized, having been exposed by its two extremities to the violet ray, in 10 minutes a north pole was formed at each of its ends.

vii. If the iron wire be oxidized, the same effect takes place in 5 minutes.

The dimensions of all the wires employed was uniformly the same as those of the one that has been mentioned. All these experiments, which, repeated several times, invariably gave the same results, have placed beyond doubt the magnetizing property of the violet ray; but I must add, that in performing them I have encountered innumerable difficulties, which have clearly shewn to me the causes of the bad success of the attempts of many philosophers. I shall not here detail the facts which have led me to general results, because that would lead to too great length, and no advantage to science would result from it, but I shall merely point out the following observations:—

i. The wire made of iron from a sulphurous mine cannot be magnetized; it is the same with iron that has been highly tempered; to which, however, I have occasionally succeeded in communicating some slight degree of magnetism.

ii. At low, or at least not high temperatures, such as – 6° R. 0°, and + 10°, only an equivocal magnetization is obtained; and it is in vain to attempt to reverse the poles of a magnetized wire, which I learned from a very long series of experiments made during last winter. While by experimenting at a temperature of + 20° Cent. as Mrs. Somerville did, or at + 25° or 26° R. as I myself did during the June and the July of last year, very remarkable results are obtained.

iii. Rather thick wires acquire a perceptible magnetism, but with difficulty.

iv. By passing the violet ray from the middle to the extremity of the needle, only weak and uncertain, if any, results are obtained.

I shall finish this note by proposing to examine if the action of the violet ray be not a chemical action. At first this action may be attributed to the feeble currents which take place between the red ray and the violet ray, and the existence of which I have several times ascertained by means of a multiplier suitably arranged; but if such were the cause of the phenomenon, the part of the wire placed under the violet ray ought, as it is evident, to acquire a south pole: now it has been seen that it constantly acquires a north pole.

It may also be thought that the magnetization in question is due to the difference of temperature of the several parts of the wire; but then, again, according to the thermoelectric law, it is the south pole which should be developed in the end exposed to the violet ray. Beside, in this case, where the temperature is the same throughout the whole extent of the wire, no magnetization should take place; now, we have seen (vi.), that a north pole is then formed at each extremity. Lastly, I shall add, that having produced artificially a lower temperature than that of the surrounding atmosphere, I observed the same effects as before, although in a less degree of intensity. These considerations lead me to think that the violet ray acts chemically. I am confirmed in this opinion, by seeing that the carburets and not the sulphurets of iron can acquire magnetic properties; that the needles artificially oxidized, present the phenomenon in question more quickly and to a greater degree than those which are not so; and that, according to the degree of temperature, the magnetic influence of the violet ray increases, becomes weaker, and evanescent. To extend my views on this point, I wished to see if I should obtain analogous effects from the light of a candle and from that of the moon. At the end of three-quarters of an hour I obtained a slight degree of magnetism in a needle exposed to the violet ray from the light of a candle; but that from the moon had no effect. I must say, that when I experimented upon the lunar ray, the temperature did not exceed + 5° R. When I shall have repeated the experiment in another season, I will publish the result.

From what is above stated, I am convinced that philosophers operating in the way pointed out will find magnetism developed by the ray, and for which neither the climate of Italy nor that of England is required, but only the precautions stated above. They will also perceive that the magnetism thus obtained is not temporary but permanent, as I have ascertained, by finding that at the end of eight months my wires and needles were still magnetic.—*Pavia, April* 10, 1829.—*Bibl. de Genève.*

17.—*Dr. Arnott's Natural Philosophy.*—(To the Editor of the Quarterly Journal.)

" Sir,—The public attention has been arrested by the statement in Dr. Arnott's new work on heat, that power is derivable from expanded air, at one fourth of the expense by which it is obtained from the conversion of water into steam.

" Dr. Arnott's inference is founded on an estimate of the quantity of heat absorbed in the production of one cubic foot of steam, and of that required to double the elastic force of five cubic feet of air. The result is deduced from the ordinary tables of specific and latent heats, and of the consequent developement of elasticity. The calculations are, I believe, correct as far as they go; but they neglect, I think, one most important consideration.

" It is proposed to heat a certain quantity of air in a certain vessel, from common temperature up to 500°, and this air so heated and expanded, having done its work, is to be exchanged for a fresh supply of cold air, to undergo the same process. But the vessel in which the air was heated to 500° will be itself equally heated, and will communicate the heat to the fresh entering air; it follows, therefore, that a full supply of cold dense air will not enter spontaneously as Dr. Arnott assumes; it must be forced in by a pump, or the vessel must be allowed to cool; if the pump be employed, the power required to fill the vessel will be at least one-half of that derived from the expansion; if the cooling process be adopted, we revert to the systems of Savery and Newcomen, in which the evaporation of a few ounces of water was accompanied by the heating and cooling of some hundred pounds of metal.

" No difficulty of this sort exists in the modern steam-engine, owing to that valuable property which allows matter in two different states, and requiring enormously different quantities of heat, to co-exist in the same place. Two contiguous quantities of differently heated matter in the same state, whether solid, liquid, or gaseous, quickly exchange temperatures. But the steam and water in an engine-boiler, though containing very different quantities of heat, remain in contact without any such tendency. Water can, therefore, be supplied to the vessel in which it is to be evaporated, before it can expand into steam, which would oppose its own entrance; while a quantity of already existing vapour could not be introduced without great difficulty. Even when the supply of water requires the aid of a force-pump, as in high-pressure engines, the water (having a bulk five hundred or a thousand times less than that of the vapour it is to form) demands a very small expenditure of power, as compared with the introduction of one measure of gas, which is to be expanded only into two.

"These remarks are offered with great deference to the author of the best epitome of natural philosophy in the English language. But the subject of steam-power is just now so exciting, and the alleged possibility of increasing fourfold the resources of Great Britain is so startling, that you will perhaps think this hasty criticism may tend to elicit a confirmation of what would be an inestimable idea, or else to guard against the ruinous attempts which might be induced by a flattering but delusive speculation.

"I am, Sir, your obedient servant, A. A."

18.—*Cement from Iron Filings.* By M. Miallre.—Having reflected about a year since upon the action of vinegar in the preparation of the cement known as *mastic de limaille,* which is prepared in this way—iron filings, garlic, and vinegar, of each a sufficient quantity to form a mass of moderate consistency; I proposed to substitute sulphuric acid diluted with water, for the vinegar, in the proportion of one ounce of acid to a litre (a little above two pints) of water, and to reject the garlic as useless. This alteration was soon adopted by all those to whom I communicated it; for vinegar generally costs in Paris eight or ten sous the litre, while the price of the acidulated water does not amount to as many centimes. Thus an architect, to whom I had made it known, assures me that this change, which at first appeared to merit no attention, will occasion a saving in Paris alone of more than 10,000 francs annually, and therefore deserves to be more extensively known. This cement is generally employed to close the joints of the stones with which most terraces are covered, &c. &c. What takes place in this operation, it is easy to see—the iron filings with which the joints are filled up, occupying a larger space in proportion as they become oxidized, the oxidation being facilitated by the action of the acid with which they are impregnated, the joints become exactly closed.—*Journal de Pharmacie—Août.*

§ II. CHEMICAL SCIENCE.

1. *On the Specific Heat of Elastic Fluids.*—This subject, which has been under investigation at various times by MM. Laroche and Berard, Haycraft, De la Rive, and Marcet, has been taken up by M. Dulong, who has applied to it a new method of investigation dependent upon the velocity of sound in the different gases. La Place shewed that the velocity of the sound in air or other elastic media, was importantly influenced and increased above the expected velocity by the heat evolved, as the vibrations producing sound passed through the air; and M. Dulong, by examining and comparing the sounds produced by different gases, has endeavoured to ascertain whether this element is the same in all of them. He arrives at this general law, remarkable for its simplicity, i. That equal volumes of all elastic fluids taken at the same temperature and pressure, when compressed or expanded suddenly by a fraction of their volume, disengage or absorb the same *absolute quantity of heat.* ii. That the variations of *temperature* which result, are in the inverse ratio of the specific heat of a *constant volume.*—*Ann. de Chimie*, xli. 113.

2. *Supposed Influence of Magnetism over Chemical or Crystallizing Powers.*—The investigation of this influence, which has been repeatedly asserted and denied, has been undertaken in a very careful and particular manner by Professor Erdmann. He first points out the number of delicate perturbing causes which may and have occasionally led to mistakes, pointing out the effects produced by irregularity in the wires—handling them with the uncovered fingers, &c. &c.; and especially states that many repetitions of each experiment should be made. The bars and magnets which he had occasion to use were very powerful, some of them competent to lift 80 pounds.

i. In experiments made to ascertain the oxidation of iron wire, even under the influence of terrestrial magnetism, it was ultimately proved, i. That the oxidation of iron placed under water is not at all influenced by terrestrial magnetism; there is no point of the horizon towards which it is more strongly or quickly produced than towards another. ii. The oxidation arising from unequal contexture of the iron always begins at the points where the wire is in contact with other bodies, not only metals, but even wax or baked earth. iii. Diffuse daylight, or the weakened rays of a winter's sun, neither retard nor assist oxidation, provided they are accompanied by no change of temperature.

ii. In experiments made with magnetized wires the results were the same; no difference of oxidation occurred at the two poles or other parts.

iii. In experiments on the reduction of metals by the humid process, as in the *arbor Dianæ* *, no influence of terrestrial magnetism could be observed. The crystallization took place in the branches of the syphon tube, and without any reference to their direction.

iv. In repeating the experiments with the additional power of a very large magnet, its poles proved to have not the slightest power over the formation or disposition of the crystal within.

v. Numerous salts were made to crystallize slowly in vessels placed over the poles of magnets, with every care that their power as conductors of heat should not interfere. The magnetism exerted not the slightest influence over the crystallizations. In chemical actions, where gas was evolved, no difference in the rapidity of evolution, or quantity of gas produced, occurred, when magnets were present or absent.

vi. No evidence of the influence of the magnetic poles over the colours of vegetable solutions could be obtained.—*Bib. Univ.* xlii. 96.

3. *On Phosphoric Acid, by* M. Gay Lussac.—Engelhart observed, that recently fused phosphoric acid, when dissolved in water, precipitated albumen, although it did not do so before fusion, and lost the property when the solution had been preserved some time. More lately Mr. Clarke discovered, that phosphate of soda, exposed to a red heat, acquired properties different from those it had before calcination. It becomes less soluble, contains less water of crystallization, is changed in form, and precipitates nitrate of silver, white instead of yellow.

These observations of Engelhart and Clarke appearing to me to have some analogy, I made certain experiments. I took liquid phosphoric acid, which had been a long while in the laboratory, and after ascertaining that it did not precipitate albumen, I saturated it with carbonate of soda. The phosphate produced, precipitated nitrate of silver yellow. Another portion of the same acid calcined, and then saturated by soda, precipitated nitrate of silver white.

Calcined phosphate of soda was then decomposed by nitrate of lead, and the phosphate of lead formed decomposed by sulphuretted hydrogen. The phosphoric acid thus produced precipitated albumen, and, when recombined with soda, precipitated nitrate of silver white.

From these observations it appears, that the remarkable alteration in properties observed by Mr. Clarke, in calcined phosphate of soda, is due to that which phosphoric acid undergoes in the same circumstances. This is still further proved by the circumstance that the phosphates of potash and ammonia made with calcined phosphoric acid, precipitate the nitrate of silver white, and that ordinary phosphate of potash acquires the same power by calcination.

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* *Quarterly Journal,* N. S. iv. p. 429.

According to these results, which I have not had time to multiply, Mr. Clarke's opinion of the cause requires to be modified. They are sufficient, however, to make us conclude that very remarkable differences exist between most phosphates before and after calcination, or between those made with calcined or uncalcined acid. It is to be remarked, that the modification of phosphoric acid by heat is much more permanent when it is combined with a base than when in aqueous solution. I hope to give other details on this subject at a future period.—*Annales de Chimie*, xli. 331.

4. *Production of Sulphuric Acid from the Vapours of the Aix Waters.*—The mineral hot waters of Aix, in Savoy, are two in number, and are distinguished, one as the aluminous, the other as the sulphuretted. It has frequently been stated that the vapours of the former contained free sulphuric acid. That they produce sulphuric acid is proved by the following observations of M. Francœur:—i. All the grottos, closed chambers, corridors, &c. where the vapours penetrate, have their walls corroded and covered with acid crystals of sulphate of lime. ii. All iron utensils, &c. are not only corroded, but often found incrusted with sulphates of iron and lime. iii. Tubes of cloth, through which the vapours are passed, are quickly rotted, and the rags are found impregnated with sulphuric acid.

No free sulphuric acid exists in the water itself; and from the circumstances it follows, that the mixture arising as vapour, and containing hydrogen, azote, sulphur, and carbonic acid, has the power of producing sulphuric acid by means of the atmosphere and in contact with the walls and metal.—*Ann. des Mines*, v. 285.

5. *Carburet of Sulphur decomposed by Voltaism.*—M. Becquerel has described an experiment on the decomposition of carburet of sulphur by low voltaic powers. Liquid carburet of sulphur was put into a tube (glass), and above it a solution of nitrate of copper: then a plate of copper was plunged into and left in both solutions. This formed a feeble voltaic pile, and chemical decomposition took place. Both the carburet and the nitrate were decomposed, much crystallized protoxide of copper was formed on the copper plate, and carbon was deposited on the sides of the tube, in very thin plates, with a metallic lustre. No formation of diamond.—*Revue Encyc.* xliii. 508.

6. *Composition of the Atmosphere at Kazan.*—The uniformity of the composition of the atmosphere taken from different places and heights upon the earth's surface is well known; but M. Kupffer still thought it advisable, when he had the opportunity, to analyse a portion of air collected at Kazan; a place surrounded on the one side by a poorly cultivated country, and on the other by the immense steppes and forests of Siberia, where vegetation is dormant for the greater part of the year. He used Volta's eudiometer, and still

found that the air contained from 21 to 21.2 per cent. of oxygen.—*Annales de Chimie*, xli. 423.

7. *Disinfecting powers of Chloride of Lime.*—M. Poutet, of Marseilles, says, that this substance cannot be used with advantage in destroying the bad odour of fish or marine animals, for that it evolves one as bad as any they can previously possess. The powder, added with a little water, to fresh or salt fish, cut into small pieces, evolved such an odour of bromine as to be insupportable. The muscle of putrid fish produced a still worse smell; and the same thing took place with other marine products, as shell-fish, sponges, &c. &c.

8. *Proportions in which Oil Gas and Air detonate.*—The oil gas experimented upon by M. Dumas, contained 18 per cent. of vapours, absorbable by sulphuric acid in a few minutes; 100 parts required 270 of oxygen for complete combustion, and produced 174 parts of carbonic acid gas. The combustion was made in a detonating Volta's eudiometer, by means of a strong spark from a Leyden jar; or on those mixtures near the limits of combustibility by a series of sparks. One volume of gas with

Air.	
1, 4, 6, and 7,	no inflammation.
8,	detonation—fuliginous flame.
9,	detonation strong—no smoke.
10 and 11,	very strong detonation—*maximum*.
12,	detonation less strong.
13,	detonation still less.
17,	feeble detonation.
18,	very feeble detonation.
20,	feeble detonation at the second spark.
21,	——————— after many sparks.
25,	no detonation after many sparks.

These results were in winter at the temperature of 40° or 42° F.—*Ann. de l'Industrie.*

9. *On the proportional Number of Lithium.*—Arfwedson, the discoverer, gives this number as 255.63 (Berzelius scale), Gmelin as 191.21 only, and Kralovansky as 254.2. This difference has induced M. Hermann, who possessed a considerable quantity of it, to examine the subject minutely. His lithia was procured from the mica of Altenberg, in Saxony, at first in the form of double phosphate of soda and lithia, which was converted into chloride of lithium, and then by Berzelius' process into carbonate of lithia, by means of excess of carbonate of ammonia. The *carbonate of lithia* thus formed was heated to redness, and found to fuse into a clear liquid, strongly acting upon platina. After cooling, it became diaphanous and crystalline, and easily broke into numerous fragments. Being analysed by Murschand, it gave 39.02 lithia and 60.98 carbonic

acid, a composition nearly the same with that of the carbonate obtained when precipitated from the muriate by carbonate of soda. Hence the lithia was proved to be free from soda, and to have 352.06 as its number.

Sulphate of lithia, formed from the carbonate and sulphuric acid, was readily crystallizable by slow evaporation. It is freely soluble in water, but not more in cold than hot. The crystals are oblique quadrangular prisms, containing 85.70 of dry salt and 15.3 of water per cent.; 100 parts of the dry salt, dissolved and decomposed by baryta, gave 74 sulphuric acid and 26 lithia. The equivalent number, according to this analysis, would be 352.1, and that of the metal lithium 152.1.

Muriate of Lithia.—When chloride of lithium is left to deliquesce in the air, large regular crystals are gradually formed of muriate of lithia. They have the singular property of becoming opaque when touched, or put upon a filter, the opacity beginning at the place touched. When the opaque crystal is pressed it falls into small crystalline fragments. The opacity, therefore, arises from a spontaneous division of the large crystals, the cause of which is not yet known. The crystals are right-angled prisms: they consist of 1 atom chloride of lithium and 8 of water, or experimentally 53.64 salt and 46.36 water per cent. When heated, the salt fuses and loses chlorine. This is in consequence of the action of oxygen, and the salt always becomes alkaline.—*Annalen der Physik.*

10. *Adulterations of the Iodide of Potassium.*—The iodide of potassium, or hydriodate of potassa, is so convenient a form of iodine, that it is constantly retained amongst medical preparations. Its utility and consequent value has led, as usual, to adulterations, one of which is described by M. Pereira, where the extraneous matter amounted to 77-hundredths of the whole. In this case the substance added was carbonate of potash; and it may easily be supposed to what an extent errors of judgment may proceed amongst medical men, when such fraudulent mixtures as these are placed in their hands by the dishonest or deceived dealer.

M. Pereira discovered the falsification whilst attempting to make a solution of iodine in the hydriodate of potassa, a preparation kept at the general dispensary. The supposed hydriodate refused to dissolve the quantity of iodine which ought to have been taken up, the carbonate present not having an equivalent power.—*Med. Jour.* lxii. 310.

11. *Preparation of red Ferro-prussiate of Potassa.*—M. Kramer has endeavoured to prepare this useful test by a process more ready, and less troublesome, than that of Gmelin. By decomposing Prussian blue at a low temperature by chloride of potassium, he obtained a liquid of a yellow colour when in small quantities, but red in larger portions; it did not precipitate persalts of iron, but

did precipitate the protosalts. By evaporation, the solution gave a little Prussian blue, and then crystals, the summits of which were quadrangular pyramids. By two or three crystallizations these were rendered perfectly pure.

The red ferro-prussiate of soda could not be obtained in a similar way, but only by passing chlorine through the ordinary ferro-prussiate of soda, formed by digesting soda on Prussian blue. By a similar process he obtained a red ferro-prussiate of ammonia, but the compound was not permanent.—*Jour. de Pharmacie.*

12. *New Method of analysing Alloys of Copper and Silver.*—This method is proposed by Professor Zenneck, of Stutgard, and is founded upon the constant quantities of hydrogen gas evolved when either silver or copper is dissolved in pure strong muriatic acid. The solution requires to be assisted by heat; and when copper is the metal operated with, the surface of the acid in contact with the air must be covered by a layer of oil; with these precautions, a given weight of copper always gives out a proportionate and equivalent quantity of hydrogen gas, and a given weight of silver also, its determinate equivalent; being in bulk less than that evolved from copper. The difference between the bulks of gas evolved by equal weights of the two metals enables a careful experimenter to determine, from a given weight of alloy and the gas it evolves, the proportions of silver and copper present.

The instrument used by Professor Zenneck is a glass tube closed at one extremity, and bent nearly to a right angle. It is expanded in the angle in the manner well known to chemists, so that when filled with liquid any acid rising from a solid piece of matter in the angle should ascend into the closed branch. It is, in fact, a bent tube pneumatic receiver. The process is fully detailed in M. Zenneck's paper, and also in the abstract given of it in the *Bibliothèque Universelle*, xli. p. 317—but we do not think it necessary to describe more than the principle. There is no doubt the process will succeed in careful hands; but objections to it, as a ready and sure method, quickly arise in the mind.

13. *Artificial Ultramarine.*—By following Gmelin's process*, M. Hermbstadt has obtained the most beautiful ultramarine imaginable. He attributes the success of the operation principally to the care taken to mix the silicate of soda and alumine in as moist a state as possible. If they are too much dried before adding the sulphur, the colour obtained is only a bluish-green.—*Allg. Handl. Zeitung*, 1829.

14. *A new Earth, Thorina.*—This new earth, thorina, is a recent discovery made by Berzelius, and must be distinguished from the sub-

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* *Quarterly Journal*, N. S. vol. iv, p. 216.

stance formerly called Thorina by the same philosopher. A new mineral body, discovered by Esmark, near Brevig, in Norway, was sent to Berzelius for examination. It was compact, black, brittle, and semi-hard, having the vitreous fracture of gadolinite, a specific gravity of 4.8, and producing a dark brown powder. Under the blowpipe it loses water, and becomes yellow. The mineral contains a new earth so like the substance formerly called thorina, as to be at first mistaken for it; when, however, distinguished by experiment, the name thorina was retained.

The new earth is colourless and infusible after ignition, insoluble in all acids except the sulphuric, and not rendered soluble in other acids by calcination with alkali. It is insoluble in caustic potash, but soluble in the carbonate; heat partially precipitates the solution, cold causes re-solution. Its salts have a pure styptic taste: a strong solution of the sulphate becomes a thick mass by boiling, but it is soluble in cold water; this property particularly characterises the new earth. Sulphate of potash produces a precipitate in the saturated solution, which is a double salt, soluble in cold water. This is a character also of the salts of cerium. Ferroprussiate of potash precipitates it, as it does yttria.

Potassium does not reduce thorina; but the chloride, obtained in the same manner as with alumina, is readily decomposed with a feeble detonation. The product is a pulverulent grey metallic mass, dissolving rapidly in muriatic acid, and but slowly in the nitric and sulphuric acids. Neither water nor alkalies act upon the metal. By friction it acquires lustre. It burns brilliantly in oxygen gas into thorina, without exhibiting fusion. Thorina contains 11.8 per cent. oxygen. The mineral (thorite), from which it was obtained, contains per cent. 57.91 thorina—18.98 silica—9.5 water—3.4 oxide of iron—2.58 lime—2.39 oxide of manganese, and portions of the oxides of uranium, lead, and tin, with traces of potash, soda, magnesia, alumina, &c.—*Hensman's Repertoire.*

15. *Analysis of Siliceous Minerals by Alkaline Carbonates.*—The ready fusion observed by M. Berthier* of many atomic mixtures of salts may be applied to the analysis of siliceous minerals by alkaline carbonates, aided by a spirit lamp. A mixture of five parts of carbonate of potassa and four parts carbonate of soda is so fusible, that between 200 and 300 grains may be rendered perfectly liquid by a spirit-lamp flame. If sand be added to the mixture, there is an effervescence as lively as if acid had been added. This effervescence occasions the expulsion of part of the substance; and by the addition of too much sand, the mass would become too difficult of fusion, unless the sand or mineral had been previously pulverised and mixed with the carbonates. Hence the operation should commence with the mixture of the carbonates and the mineral. In this manner considerable quantities of felspar may be readily de-

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* *Quarterly Jour.*, N. S. vol. iv, p. 436.

composed by the heat of a spirit of wine lamp.—*Annalen der Physik*, 1828.

16. *Gay Lussac on the Action of Potassa on Organic Matters.*—M. Vauquelin, by treating pectic acid with potash in a crucible, converted it into oxalate of potassa. This experiment suggested to me the idea of submitting ligneous matter, which has an analogy to pectic acid, to the same treatment, and I obtained the result I expected.

5 grains of cotton were put with 25 grains of pure potash and a little water, into a platina crucible, and heated over a spirit lamp much beneath redness. The cotton resisted the action of the alkali at first, but ultimately softened, the mixture melted without undergoing carbonization, and hydrogen was disengaged. During the tumefaction the mixture should be continually stirred. When it had settled down, the mass was dissolved in water, and rendered slightly acid by nitric acid. It then gave an abundant precipitate with nitrate of lead, and this, operated upon by sulphuretted hydrogen, produced very fine crystals of oxalic acid. With nitrate of lime, a voluminous precipitate of oxalate of lime was obtained.

Wood sawdust, with the same treatment, gave the same result.

Sugar, with 4 or 5 times its weight of potash, became, when heated, at first brown, then white, and gave much oxalic acid.

Starch formed a very glutinous mass with potash, which long retained this state. More alkali occasioned liquefaction, the mixture swelled, and oxalate of potash was produced.

Gum and sugar of milk were also converted into oxalic acid with the disengagement of hydrogen.

The most remarkable transformation is that of tartaric acid into oxalic acid. There is no swelling, no blackening, and (which merits particular attention) so little evolution of hydrogen, that it may be considered as due to extraneous vegetable matter. When the hydrogen is to be collected, the experiment may be made in a retort to which a tube of glass has been attached, which is to be plunged beneath a layer of water into mercury to prevent absorption. The retort being heated by a bath of mercury or oil, it will be readily observed that a temperature of 400° F. at most is sufficient to form the oxalic acid.

Citric and mucic acid produced also much oxalic acid. I have also obtained it from succinic acid, but the benzoic acid resisted the action of the potassa, and remained unaltered.

Acetate of potash heated with excess of potash became converted into carbonate. A little oxalic acid was obtained, but it is very probable that it was due to extraneous vegetable matter.

Colza oil, notwithstanding a great excess of potash, could not be brought into fusion, and but very little oxalic acid was obtained.

Amongst animal substances, silk, treated with potash, gave oxalic acid with disengagement of hydrogen. Uric acid evolved ammonia.

The residue was very white; being dissolved in water, and saturated by nitric acid, hydrocyanic and carbonic acid were disengaged; nitrate of lime then produced an abundant precipitate of oxalate of lime. Gelatine gave a similar result. Indigo gave no oxalic acid.

Carbonate of potash used instead of caustic potash gave no oxalic from tartar; nor did lime and starch produce any oxalic acid. Soda may be used instead of potash.

From these experiments, it appears that a great number of animal and vegetable substances, acted upon by caustic potash or soda, are transformed into oxalic acid. It is to be remarked that the formation of this acid precedes that of carbonic acid, and precisely under the same circumstances, as when sulphur and potash, for instance, produce hyposulphurous and sulphuric acids. Thus a vegetable substance, heated moderately with potash, gives oxalic acid, but when more strongly heated, carbonic acid.

As very different substances produce oxalic acid, it is necessary that other products should be formed. Many bodies evolve hydrogen which may come from themselves, or from the water they contain, and afterwards carbonic acid. Animal matters, besides these two products, give also ammonia and cyanogen. Water must also be formed with both animal and vegetable substances. These products, or some of them, are sufficient to explain in general the formation of oxalic acid, but in some particular cases there ought to be other products obtained. Thus tartaric acid gives no sensible portion of hydrogen, and yet its composition being $2\frac{1}{2}$ proportions of hydrogen, 4 of carbon, and 5 of oxygen, we cannot explain its transformation into oxalic acid by the occurrence of any of the known products.

In fact, during the operation, the mixture remains perfectly white. If all the carbon entered into the oxalic acid, it would require 6 proportions of oxygen, and consequently water ought to be decomposed to furnish 1 proportion. If only so much oxalic acid were formed as is proportional to the oxygen in the tartaric acid, then $\frac{2}{3}$ of a proportion of carbon would remain, which might form a particular compound with the hydrogen; and for 1 proportion of tartaric acid, $1\frac{2}{3}$ of oxalic acid would be produced. In place of this last quantity, I have obtained $1\frac{1}{3}$ oxalic acid, but I have not discovered any hydrogenated compound. Finally, it was possible that a peculiar acid had been formed by the carbon, oxygen, and hydrogen. This point deserves particular examination, and I should have undertaken it in the vacations, if I had had time, but hope to resume the subject shortly.

I shall conclude by describing a very elegant method of transforming tartar into oxalate of potassa. It consists in dissolving rough tartar in water, with a proper quantity of potash or soda, and making the solution pass by means of a pump in a continual current through a thick tube of iron or bronze heated to 400° or 450° F.

The pressure need not be more than 25 atmospheres, for no gas will be disengaged. A valve is to be placed at the opposite extremity to that at which the solution enters, and charged with sufficient weight to obtain this pressure; it will then only be opened by the pressure exerted by the injection pump. I have not as yet tried this process, which is also applicable to other substances, but I see nothing which can prevent its success. According to some experiments which I have made, less than a proportion of potassa for a proportion of neutral tartar will be necessary.—*Ann. de Chimie,* xli. 398.

17. *New Source of Spirit.*—It is stated that the berries of the *Sorbus Aucuparia* are now used in the north of France for the production of a spirit, and the result is said to be equal to the purest distillation from grapes for brandy. The berries, when perfectly ripe, are first exposed to the action of cold in the open air, then put into a wooden vessel, bruised, and boiling water poured on, the whole being stirred until it has sunk in temperature to 82° F. A proper quantity of yeast is then added, the whole covered up and left to ferment. When the fermentation is over, the liquor is to be put into the still, and drawn over in the usual way. The first running is weak and disagreeable in flavour, but being distilled from off very fresh finely powdered charcoal in the proportion of 8 or 9 lbs. to 40 gallons of weak spirit, a very fine product is obtained. The charcoal should remain in the liquid two or three days before the second distillation.

18. *Effect of Ether on Sulphate of Indigo.*—M. Cassola states, that when sulphuric ether is added to sulphate of indigo, in about half an hour, at the temperature of 100° F., the colour of the indigo totally disappears, and no substance whatever is capable of restoring it. The colourless mixture being subjected to distillation, yields a liquor which reddened litmus strongly, and gave no precipitate with barytic salts; but with nitrate of silver a precipitate was obtained soluble in ammonia.—*Hensman's Rep. Phil. Mag.* N. S. vi. 393.

19. *Composition of Malic Acid.*—M. Frommherz has endeavoured to settle the discordances in the experimental estimation of the proportions of carbon, oxygen, and hydrogen, in malic acid. He burnt the malate of lead by peroxide of copper. His malate consisted of 39.375 malic acid, and 60.625 oxide of lead. From the mean of three experiments, he deduced that the acid contained 6 atoms of oxygen, 7 atoms of hydrogen, and 3½ atoms of carbon, or

Carbon . .	29.357
Oxygen . .	65.863
Hydrogen . .	4.780

Then it is remarked that a half atom is contrary to all theoretical ideas, and *therefore* that malic acid must contain just double the number of atoms given above, or else that carbonic acid is formed of an atom of carbon and an atom of oxygen. We recommend the facts as useful; the theory merely as amusement. What is an atom!

20. *Chemical Constitution of Acetic Ether.*—By a series of experimental researches, M. Planiava has arrived at the conclusion, that acetic ether is formed of 1 equivalent of acetic acid and 2 equivalents of alcohol; that, therefore, it is a sub-acetate of alcohol, and is represented by the number 97.—*Kas. Archives.*

21. *New Vegeto-Alkalies obtained from Cinchona.*—Dr. Serturner, in re-examining the products obtained by chemical means from the cinchonas, finds that the precipitates produced by alkalies from the acidulated infusion of these barks contains, besides cinchona and quinia, other vegeto-alkalies, which are to be considered as modifications of the former. The new bodies recall the case of opium to mind, in which narcotine exists simultaneously with morphia. The new substances, and especially that named by M. Serturner *chinioïdia*, exist in the alkaline precipitate, in intimate combination with a resinous subacid substance, which is not injurious, but is of no advantage. It is very difficult to separate these two substances, and M. Serturner succeeded only when he used the charcoal obtained when croconic acid is prepared by Liebeg's process. This substance, combined with animal charcoal, completely decolours the solution of the alkaline matter in sulphuric acid (diluted with 3 or 4 parts of water), but it is necessary afterwards to act on the thick solution with alcohol, to separate earthy salts.

The new vegeto-alkalies exist in the red and yellow cinchona with the quinia and cinchonia. The chinioïdia has more alkaline power and capacity of saturation, and also more medical power than any other vegeto-alkali in the cinchona, but it resembles them by its insolubility in water, its colour and taste. Its alkaline reaction on known vegetable colours, and its intimate state of combination with the brown extractive matter, are remarkable. Its salts are very fusible by heat, and become viscid like some balsams.

According to M. Serturner, in febrifuge power, chinioïdia is as superior to quinia and cinchonia as these are to ordinary bark. It is to this alkali that many cinchonas are indebted for their medical powers. M. Serturner has, in many cases, given his new medicine in doses of 2 grains three times per day; the patients take a little vinegar after each dose, for the purpose of saturating the gastric juice, which, by its alkaline nature, would else decompose the salt: from 12 to 24 grains have, in all the cases, sufficed to prevent the return of the fever, whilst patients, in the same neighbourhood, treated with the sulphate of quinia, had frequent returns of the disease.—*Hufeland's Journal.*

22. *Investigation of Tobacco—Nicotia.*—It has been two or three times supposed that a vegeto-alkali has been obtained from tobacco, to which its peculiar properties are due. Lately MM. Posselt and Reimann have resumed the investigation, and think they have obtained the true principle by two processes. i. Mix 1½ lb. of tobacco with 2 ounces of potash and enough water; distil in a glass retort; add fresh water to the residue in the retort, and again distil; repeat this two or three times; after which the leaves will be found deprived of their acrid property, and harmless if taken internally. The united products are to be neutralized by sulphuric acid, and evaporated nearly to dryness; the irregularly crystallized brown mass is to be acted upon by strong alcohol, the solution diluted with a little water, and distilled. The brown aqueous residue is to be treated with concentrated potash, and again distilled, when a clear light-coloured oily substance will be obtained, very acrid, with an odour which, though slight at common temperature, becomes insupportable by elevation of temperature. Ether added to this substance in successive portions takes away the acrid principle, and the solution, when distilled, leaves the pure principle in a very concentrated state. ii. Or boil 12 lb. of dry tobacco leaves in water, acidulated with sulphuric acid, evaporate, and treat the residue with alcohol, diluted with a ninth of water; add a little water to the solution, and distil; add hydrate of lime to the aqueous residue, and redistil; the product being mixed and agitated with ether, the latter is to be poured off, and a fresh portion added as before. All the ethereal solutions are to be conjoined and put in contact with muriate of lime, which will take away the water; and the concentrated ether solution being evaporated or distilled, will leave 2 gros or 118 grains of reddish brown *nicotia.*

Pure nicotia is limpid, and liquid at 21° F.; its odour resembles that of dry tobacco; its taste very acrid, burning, and enduring. It stains paper, but the mark disappears in some hours: it is heavier than water, volatilizes in the air, and boils at 474° F. It burns round a wick, and produces white vapours at 212°F. It dissolves in water in all proportions, and the solution has an alkaline reaction. It dissolves in alcohol; but when this solution is distilled, the nicotia does not pass over. Ether dissolves it in any quantity, but neither does its vapour carry any portion up. Acids take the nicotia from these ethereal solutions, and form salts insoluble in ether. The phosphate of this substance is crystallizable with difficulty, forming a substance having the appearance of cholesterine. The sulphate is uncrystallizable; nitric acid destroys the body in part; oxalic and tartaric acids form crystallizable soluble compounds.—*Geiger's Mag.*

23. *Preparation of Urea, by* M. Henry.—Let a slight excess of the subacetate or the hydrate of lead be added to recent urine; a precipitate will fall which will contain salts formed by the union of the acids in the urine with oxide of lead, and also a combination of

the mucus and animal matter present with the hydrate or subsalt used.

The clear fluid is to be acted upon by diluted sulphuric acid, added until in slight excess, to separate the lead present, and to act, during the future evaporation, upon the acetates of soda and lime which may be formed. The liquid is again to be freed from the precipitate, and quickly evaporated, animal charcoal being added to it during the ebullition. When clear the fluid is to be strained through a fine cloth, and concentrated to one-third of its bulk; on cooling, it will probably become a yellowish acicular crystalline mass, consisting of much urea and some salts. The crystals, when drained and pressed, are to be added to those produced by evaporating the mother water, also similarly treated; being thus freed from the brown viscid matter which previously accompanied them, they are to be treated with a small quantity of carbonate of soda, to decompose any acetate of lime which may remain, and then are to be digested in alcohol. The solution, filtered and distilled, leaves urea, which may be recrystallized by solution in water and evaporation.—*Journ. de Pharmacie, April*, 1829.

24. *Composition of different Bones.*—The following account of the earthy part of different bones, as given by Dr. F. de Barras, is interesting. The quantities are those obtained from a thousand parts of bone.

	Sheep.	Hens.	Fishes.	Frogs.	Lions.
Carbonate of lime. . .	193	104	53	24	25
Phosphate of lime. . .	800	836	919	952	950

—*Jameson's Journal.*

25. *New proximate Principle from Albumen, by* M. Couerbe.—White of egg was left to itself at a temperature of 17° or 18° F. It did not congeal, but thickened a little, and at the end of a month gave an abundant membranous net-work and a liquid matter, no putrid gas having been disengaged. The liquid was slightly examined, and by its decomposition gave carbonate of ammonia; hence it was concluded that the liquid contained the animal part of the albumen.

The membranous substance was white, translucid, and of a foliaceous structure, insipid, inodorous, and friable. Heated in a tube, it did not fuse, but was decomposed, swelling at the time, evolving no azoted products, but leaving a voluminous light charcoal difficult to burn. When decomposed by oxide of copper, it gave only water and carbonic acid. In cold water it did not dissolve; in boiling water it softened, melted, and looked like insoluble mucilage. Alcohol, ether, and acetic acid exerted no action upon it, hot or cold. Sulphuric acid exerted little action at common temperatures; but by heat carbonized it, evolving an agreeable aromatic odour. Nitric acid acted but little when cold; by heat dissolved it, evolving nitrous gas. Muriatic acid dissolved it when heat was applied, and the solution remained clear on cooling; by dilution, a fine white powder was deposited.

Potash, with heat, dissolved it, and rendered the solution turbid, but did not cause any deposition in 24 hours.—*Annales de Chimie*, xli. 323.

26. *To ascertain the Admixture of Sulphate of Copper in Bread.*—In the *Observateur de Liège*, MM. Meylinck and Hensmans have given three processes for ascertaining the presence of this salt in bread—the humid way—the dry humid—and the dry. The *dry humid*, which they prefer, consists in letting fall a drop of ferro-prussiate of potassa on a slice of bread; whether or not there is sulphate of copper in the bread, a reddish spot will be formed if the bread be fresh: a blue one, if the bread be sour. Immerse the bread in lime-water, the spot will scarcely change colour, or will become yellow if there be no adulteration; but, on the contrary, will assume a greenish tint if it contain sulphate of copper. In the second case, expose the bread to the action of ammoniacal gas, which will immediately change the blue colour of the spot to a pale citron yellow if the bread is pure; on the contrary, if it be adulterated, the spot first becomes red, then yellow, and from the yellow it may be brought back to the red, by volatilizing the ammonia, or exposing it to muriatic vapour. By the *humid way*, we may proceed either, i. By treating the bread with acidulated water, passing the liquor through a flannel, and then examining it by the ordinary re-agents after having neutralized it by ammonia; or ii. By treating the bread with concentrated alcohol, boiling; the alcohol takes up the salt of copper, and a solution is thus obtained, which is to be treated with the ferro-prussiate. The *dry way* consists in burning the bread in an unvarnished earthen vessel. A stratum of different coloured ashes is thus obtained, covered with an efflorescence of a mixed green and blue colour. According to M. H. it is preferable to treat separately the ashes and the efflorescence; but Dr. Jacquemys, of Liege, regards this as useless, because the copper contained in the whole mass may easily be dissolved in nitric acid. The solution having been effected, it is to be neutralized by ammonia, and enough ammonia added to precipitate the alumina from the flour and dissolved by the acid. The presence of copper may thus be ascertained by the hydroferro-cyanate of potass. To determine the quantity of salt contained in the bread, it is necessary, after having precipitated the alumine, to filter the liquid; add to it prussiate of potass and of iron, then dry and weigh the precipitate, and from this datum calculate the quantity of sulphate of copper.

27.—*Letter from Sig. Carlo Matteucci of Forli to Professor Gazzeri*:—

"Sir,—The importance of the new fact, in solar electricity, which I communicated in my letter published in April, in the Anthologia for this year, obliges me to make known, on the same subject, some observations not less interesting than the former. From the time when I first observed this phenomenon, I have adhered to the idea, that the electric state acquired by the glass might depend on the evaporation of the aqueous film, which always covers its surface. Therefore, to remove so doubtful a fact from my observation, I endeavoured to perform the experiment in a different manner. After having frequently touched a flat piece of glass with a trial plane, without perceiving any sensible developement of electricity, I heated it strongly that it might get rid of the moisture adhering to it; and having then left it to get dry under a bell, which had been previously dried by heat and by muriate of lime, I then frequently made trial with the same plane, and never did I see any electricity developed by it, which, however, would have quickly happened if the plate had been abandoned for a few moments to the direct action of the solar rays. Yet, however frequently I saw the evaporation of the stratum of water which covered it, excluded from the causes of rendering electric the plate of glass exposed to the sun's rays, still the touching of the glass plate with the trial plane did not appear to me a method free from all exception, it being too easy to develop electricity either by pressure or by any friction. I wished, therefore, to vary my method of experiment, so as not to have my observation, if correct, lie open to denial. Having added to the condensing plate a metallic wire, soldered at the extremity to a large disc of brass, I placed upon this a glass plate, and made the sun's rays fall upon it, without their touching the box of the electrometer. I then perceived the leaves diverge sensibly, and having raised the plate, and then the collecting plate, I observed, as was very natural, the divergence increase. In this way, the cause of the electric state of the glass exposed to the sun's rays appeared to me reduced exclusively to the power of those rays. If these new statements appear to you interesting to science, they are at your disposal."—*Carlo Matteucci, Forli, August* 13, 1829.

28.—*On a new Oxide of Manganese.* By Mr. Phillips.—Mr. R. Phillips, whilst engaged in examining the oxide of manganese from Warwickshire, was enabled to distinguish certain parts of it from other oxides with which it had been confounded, and to shew that it was a new compound of the metal with oxygen. He has called it *Varvacite.* In general appearance, and many of its properties, it so closely resembles the native peroxide, that there can be little doubt but that it has frequently been mistaken for it.

This mineral has a grey colour, the tint of which is not remarkably different from that of the well-known crystallized peroxide; it is, however, less brilliant. It is much harder than the peroxide, does not soil the fingers so much, and is lighter in the proportion of 4.283 to 4.819. When reduced to powder and boiled in water, a trace of muriate of lime is discoverable.

It loses nearly the same by exposure to strong red heat as the peroxide, *i. e.* about 13.26 per cent.; but on examination, it appeared the proportion of oxygen evolved was much less than when peroxide was heated, and the quantity of water more. By close analysis it appeared to consist of,

Manganese	63.
Oxygen	31.6
Water	5.4
	100.0

Or two atoms deutoxide, three atoms peroxide, and one atom water.

Mr. Phillips gives the following view of the known compounds of manganese and oxygen :—

	M. O.	M. O.
Protoxide . . .	1+1	28+ 8
Deutoxide	2+3	28+12
Peroxide . . .	1+2	28+16
Red oxide	3+4	28+10.66
Warwick oxide. . .	4+7	28+14
Manganous acid . . .	1+3	28+24
Manganesic acid . .	1+4	28+32

So similar is the new compound to some of the old well-known compound of manganese, that at first some difference of opinion as to its nature took place between Mr. Phillips and Dr. Turner; which has, however, been removed by experiments made by the latter, with a true specimen of varvacite.—*Phil. Mag.* N.S., v. 209, vi. 281.

§ III. Natural History.

1. *On the Direction of the Roots and Stems of Plants.*—This subject has been examined closely by M. Poiteau, who, in his Mémoire, considers several facts, and some theories. He first refers to the well-known experiment by Mr. Knight, in which seeds, made to grow on the circumference of a revolving wheel, threw out their radicles in an inclined direction from the centre of motion, and their plumula in an opposite and equal inclined direction towards the centre of motion. This effect, M. Poiteau says, is simply due to the exertion of a physical law, namely, that when a heavy body is projected into space by any force, as the centrifugal force, in the experiment under consideration, the heavy extremity precedes the lighter. Now, the radical extremity is of greater specific gravity than the leaf end of the plant, and consequently, in the wheel ex-

periment, tends outwards. The experiment, therefore, has nothing to do with the natural direction of roots and stems.

Another theory considered is, that which views a tree or plant, not as a simple individual, but as a union of universal independent parts, arising each from a bud; the bud being then considered as a seed, which, under developement, produces latent roots that descend, or tend to descend, towards the earth, and constitute the fibres of the wood. In support of this theory, M. Poiteau quotes the *Rhizophora*, growing on the borders of the Mahuri at Cayenne; the trunks of which produce roots at different heights, and are obliterated at the parts inferior to these roots, because, according to M. Poiteau, the fibres of the lower buds no longer descend to the bottom of the tree, but become and constitute these aërial roots. The same is the case nearly with the *Ludovia funiculifera*, a plant of Guiana, described by M. Poiteau. Many palms have their stipes sustained by aërial roots, of which the most recent are the lowest: to explain this fact, M. Poiteau remarks, that the growth of the plant is from without inwards, from which he concludes that the youngest woody fibres, being situated within, their prolongation should be less than that of the more ancient fibres.—*Bull. Univ.* D. xiii. 74.

2. *On the Nature and Character of the Potato Root, and other Vegetable Bulbs.*—A particular investigation of the internal and external organization of the *Solanum tuberosum* and *Helianthus tuberosus* has been undertaken by M. Turpin, in consequence of which he has been led to lay down the particular distinctive characters of the stems and roots of plants, and to class the two tubers above mentioned with some others as true vegetable stems.

The essential character of roots, in whatever medium the latter may be developed, is the entire absence of vital nodes, or of those points generally expanded and disposed symmetrically, and in determinate situations on the surfaces of stems, and consequently of the foliaceous appendages which always accompany buds. The multiplication of their fibres is always adventitious, has no determinate regularity, and may take place from any part of their surface.

The essential character of stems, whatever the medium in which the latter are developed, is the occurrence of vital nodes, disposed symmetrically, and constantly bordered, or accompanied by a fibraceous appendage, which is sometimes reduced to its rudiments, or, indeed, almost absorbed. Buds and bulbs grow from these vital nodes, which are their true receptacles.

A long and accurate series of observations are then gone into, by which it is shewn, that the roots of the descending parts, and the adventitious fibres of the potato, never do, under any circumstances, thicken, so as to produce a tubercle, such as could be called a potato; and that the same is the case with the *Helianthus tuberosus*, or *Topinambour*.

True stems, which arise from the lowermost vital nodes or buds upon the mother stem of the plant, when they travel and lengthen themselves under ground, and become thickened at their extremities, produce the tubers of the potato and *topinambour*. The potato, like a stem, can ramify, and has produced successive ramifications below the surface of the ground up to four generations, all of them apparent, and produced one upon another. These four generations are rigorously comparable to the branch of a tree, on which were the shoots of 1826, 1827, 1828, and 1829.

M. Turpin then describes three sorts of existences, or modes of being, or three individual systems, which are observed by the microscope, as conjoining to form, by simple agglomeration, the substance of a potato. The first existence is found in the prodigious quantity of distinct mother vesicles, white, soft, transparent, and nearly spherical, which, being placed in an irregular way, with respect to each other, have insignificant and irregular spaces between. The mass of these vesicles is called, in vegetables, the cellular tissue, and in potatoes is, in fact, the starch. In those which are ovaria, exist others, representing the future cellular tissue; the latter are called globuline. The second system consists of certain internal fibrils, either simple or compound, which originate, extend and vegetate amongst the cellular tissue; these constitute the vascular tissue. The third system exists in the general membrane which invests the whole, and incloses, but without limitation, the vesicles and the fibrils: this is the cuticular system.

These three existences have a perfect individuality, and never change their nature; but the cuticle is remarkable, for that nature seems to have placed in it the power of restraining, in every way, the blind and uncertain developements of vesicular or vascular systems which vegetate beneath it.

Each vesicle of the cellular tissue, and each fibril of the vascular system, has its particular vital centre of vegetation. Each of these elementary systems lives, increases, and propagates upon its own account; at the same time that it is subjected to make part of a more compound individuality—namely, that of the plant.

This multiplicity of particular lives, or distinct individuals, in the composition of the masses of vegetables, explains how the life of a plant is equally spread over the whole of the living part; and how from any of these points may be developed the germination of an adventitious embryo, and consequently of a new plant.

M. Turpin then applies similar reasoning to animal systems, and endeavours to shew there, also, the existence of independent vital centres, or, of individualities which, though quite distinct, are made to hold the place naturally belonging to them in the complicated animal system; and in thus viewing the subject, he says, "I think I have touched the most important point in the organography and physiology of organized beings."—*Mém. du Muséum*, xix. p. 1.

3. *Peculiar Cultivation of Potatoes.*—A French soldier placed half a dozen potatoes at the bottom of a cask upon a layer of sand and fresh earth, three or four inches thick; when the stalks had risen a few inches, he bent them down and covered them, four or five inches deep, with the same mixture. He continued this operation until the cask was full. Six or seven months after, upon emptying the vessels, (which stood in a court-yard,) he found that the half dozen of potatoes had produced an enormous quantity of new ones from the portions of the mother stems which had been successively laid down and covered.—*Jour. des Connais. Usuelles*, 1829, p. 66.

4. *Curious Phenomenon in Vegetable Physiology.*—M. Alix, of St. Valery, near Somme, has in his possession an apple-tree the source of which is unknown, and the age supposed to be forty years. This tree, which perfectly resembles the ordinary apple-tree in its leaves, and the disposition of its flowers, differs by the flowers being deficient of petals and stamina, but possessing instead fourteen styles and a calix with ten segments, connected below, but disposed in two alternate ranges. The peduncle of the flower is woolly, and the styles, being slightly hairy at the base, are surmounted by a very viscid oblique stigma. In consequence of the organization of these flowers, the tree was sterile, until, it having been suggested that artificial fecundation could be effected by means of pollen taken from other apple-trees, the tree was made to produce fruit. Since then, it has become a sort of festival in the country to render the tree productive; and those of the neighbourhood who feel an interest in the progress of the tree, when it comes into flower, so soon as they meet with a perfect apple flower elsewhere in fine dry weather, they pluck it and apply it to one of the flowers of the sterile tree, and leave it there until it falls off; then the person distinguishes the flower by attaching a coloured ribbon to it, so that in the autumn each may know his apple.

The apples differ much from each other in their size, taste, and colour, because of the variety in the trees from which the perfect flowers were taken; but they are all distinguished by a degree of contraction, situated nearly about one-third from the end. Within each apple are fourteen cells, situated in two horizontal and parallel planes; five of these are disposed as in the ordinary apple, in the middle of the fruit; but the other nine, which are smaller, are nearer to the top of the apple. Each cell does not always contain a seed; the number of the latter varies from three to nine. The arrangement of these cells has some analogy with the appearance which two apples would present if fastened end to end, and of which the longitudinal section would present the figure of a leaf in the shape of a violin or panduriform.

Wildenow, Poiret, and others, have described unisexual apple-trees, in which the petals and stamina were absent; but they differ from the tree of St. Valery in their fecundation being effected by the vicinity of other apple-trees, and in having only a five-leaved calyx, from five to ten styles, and five cells in the fruit. M. Tillette de Clermont explains the present case, by the theory of junctions and miscarriage developed by M. De Candolle. In applying this theory to the tree in question, it must be imagined that there is the flower of an ordinary apple-tree, from which are developed two other flowers, which, instead of being supported on separate peduncles, must be considered as joined together, and at the same time conjoined with the simple flower from which they have their origin; and this in such a manner, that the soldered ovaria of the two upper flowers are superposed and soldered to the ovarium of the inferior flower, a style and a cell being at the same time suppressed. This natural monstrosity, therefore, is the product of three flowers soldered together, in which there is at the same time suppression of the petals, stamina, a calyx, and a pistil. The examination of the fruit leaves no doubt on this subject, and evidently proves the truth of M. Clermont's hypothesis.—*Revue Ency*. xliii. 762.

5. *Effect of Iodine upon Germination.*—A series of comparative experiments have been made by M. Cantu, upon the germination and vegetation of plants moistened with water, solution of chlorine, and solution of iodine; the latter of equal density. The following are his conclusions: i. Iodine is generally more effectual than chlorine in facilitating the germination of seeds; ii. Iodine produces this effect by stimulating the germen of the seeds in the same manner as oxygen and chlorine; iii. Iodine is absorbed by the growing plant, but, by its affinity for hydrogen and the power of vegetation, is soon converted into hydriodic acid; iv. The germination of seeds, which appear to have lost all vital power, may frequently be excited by iodine.—*Bull. Univ*. D. xii. 74.

6. *Preservation of Seeds.*—M. D'Arcet has preserved corn, which had been infested by weevils, for a considerable time, by putting it into vessels previously filled with sulphurous acid. All the weevils perished, and the corn ceased to suffer. In this manner insects in seeds may not only be destroyed, but their presence prevented. As it might be inconvenient to burn sulphur in the vessels to be filled with sulphurous acid, we will indicate another method of replacing the acid, and obtaining the same results. All that is necessary is to powder the seeds well with flowers of sulphur, before they are put into the bottles or other vessels; or, after having put the seeds into a bottle, the sulphur may be added, and the whole well shaken together, so as to bring it into contact with all the seeds. The presence of the sulphur will prevent entirely the attacks of insects.—*Journ. des Connais. Usuelles*, 1829, lxviii.

7. *New and hardy kinds of Olives.*—Two new species of the olive have been discovered in the southern district of the Crimea; this discovery will render it practicable to rear this useful tree in much more northerly climes than has been hitherto possible. The shoots, which were planted in the botanical garden of Nikita, have lived through one of the hardest winters ever known, though the severity of the weather would have been fatal to the Italian or French olive.—*New Monthly Mag.* xxvii. 438.

8. *On a dangerous Plant growing among Water-Cresses.*—The procumbent water parsnep, or *Sium Nodiflorum*, is a dangerous plant of the umbelliferous class, which grows mixed with water-cresses in springs and streams; when not in flower it so much resembles the latter, that it is with difficulty distinguished except by a botanist. Water-cresses are of a deeper green and sometimes spotted with brown, and the extremities of the leaves are more round, and especially the last leaves, which are in pairs larger than the others, and undulated at their edges. The water-parsnep, on the contrary, is of an uniform green, the ends of its leaves are longer and narrower, conical at the extremities, and toothed at the edges. The best method of knowing them well is, to examine them in July, when their flowers are expanded, and when they may be thoroughly distinguished from each other.

9. *Habits of the Egyptian Scarabæus.*—From the Notes of a Traveller in the Libyan Desert: "October 12th. Being on watch this night, I caught, for the first time, the *Scarabæus ateuchus sacer*, or chafer, with which the imaginations of the ancient Egyptians so frequently busied themselves. My attention was attracted by a noise close to my side, and athwart the darkness I discovered a large rolling ball. Conceiving it to be a crab or land tortoise, I took it into my hand, but found it to be nothing but a lump of horse-dung, and immediately afterwards I perceived a similar ball come rolling towards me. Upon holding my lantern down and minutely examining this strange machine, I found that it concealed a large black chafer, who drove it forwards by means of his long hind legs; and as it proceeded it gradually increased in size by the continual accumulation of sand; this indeed became so considerable at last, that the insect itself was scarcely perceptible. It is more than probable, that the Egyptian priests took advantage of this deception to mystify their followers, and that their veneration for the chafer, or scarabæus, arose from that circumstance. Upon a further examination, with the aid of my lantern, I discovered several animated balls of a like description more than three inches in diameter. My Arabian companions, however, did not appear to take notice of them."—*N. M. Mag.* xxvii. p. 482.

10. *Method of killing Insects for Preservation in Cabinets.*—This

method consists in inclosing the insect in a paper or thin wooden box, and exposing it for one or two seconds to heat near the fire. The heat immediately kills insects the most tenacious of life. The process does not alter the most delicate colours; but if the heat be continued too long, the wings and other parts of the body begin to wrinkle.—*Bull. Univ.* B. xviii. 312.

11. *New Applications of Chloride of Lime.*—This substance is daily finding new applications. In France, a very important one appears to be, its application to silk-worm houses. A moderate use of it in some very strong cases appears to have preserved the worms in health even when they were purposely put in contact with diseased worms. As an ordinary application, it seems to be sufficient to put about an ounce of the bleaching powder into two pints of water for each quantity of worms obtained from an ounce of eggs. This is to be put into a pipkin in the middle of the place where the worms are, the mixture agitated, the clear liquor withdrawn, and sprinkled about the place two or three times in each twenty-four hours, according to the state of the air. Fresh water is to be added in a similar way to the undissolved portion, which is not to be rejected until it has lost its peculiar smell.

12. *Preservation of Meat and Fish by Means of Ice.*—Some experiments have been made by order of the Council of Health of the Prefecture in Paris, relative to the preservation of meat and fish by means of ice. The experiments have been varied, both as to the time during which the application of cold was continued, and the nature and state of the substances operated with. The results were as follows:—i. Fresh meat of every sort, and also fish, may be preserved in ice for a long time without suffering any alteration. ii. The placing these substances in ice, when in a putrefactive state, will stop the decomposition. iii. Substances which have been put in a fresh state into the ice, and kept there for a longer or shorter period, when withdrawn and exposed to the action of the air putrefy with great rapidity; if the temperature is rather considerable, even a few hours will suffice to bring on putrefaction, and render the substances unfit for food. iv. When these substances are cooked immediately after being taken from the ice, they not only do not lose any of their good qualities, but are more tender and delicate than if they had not been so preserved.

13. *Torsion of the Arteries.*—M. Amusat has lately described to the Academy of Medicine at Paris, a process which he proposes to be used in place of ligatures to the arteries and veins, when these vessels are divided. The method consists in subjecting the artery to torsion, and has succeeded beyond his expectation. It is as follows:—When the artery has been exposed and cut, its free extremity is to be laid hold of by a small pincers, which close on

it by a spring; then applying more or less force, the artery is to be drawn until five or six lines in length have been separated from the flesh; then, by means of a second pincers, the surrounding tissues are to be forced upwards or downwards, so that the artery shall be perfectly isolated from the nerves and other soft parts, which might have been laid hold of with it. That done, a rotatory movement is to be impressed on the artery by revolving the pincers between the fingers, and this is to be continued until the portion held by the instrument is broken; the hæmorrhage will then be arrested. When an artery is to be twisted, it is well to fix it between the finger and thumb of the left hand, and then four or five turns will be sufficient to break it. When it is left free, the torsion extends up it to the next branch, more turns are required, and the operation prolonged. When the twisted artery is left to itself, it becomes tense and vibrates in accordance with the pulse; when dissected, the twisted part is found to be solid, and the inner coats are found to be broken, as in the application of a ligature; but besides they are puckered, and form a sort of *cul-de-sac*, or valve, against which the blood presses.

M. Amusat has made many experiments upon this mode of closing arteries, with great success. He has applied it twice in human beings; once in a case of amputation at the knee, and once in a case of extirpation of the testicle; in both it was perfectly successful. In his experiments upon dogs, he has tied the crural artery on one side, and twisted that on the other: two of the animals died from hæmorrhage occurring on the side where the ligature had been applied. One advantage urged by M. Amusat in favour of the process is, that the artery is always operated upon free from the nerves and neighbouring parts, whilst ligatures often inclose these parts and produce serious evils; he adds, also, that ligatures are rarely drawn tight enough, or well practised. He thinks that the process may be substituted, with advantage, for that of tying; i. Because it is simpler; ii. Because a single person can do it, requiring no assistance—an advantage of great importance in pressing cases, either in the country, or in battle; iii. Because it offers great advantages in the immediate re-union of the parts affected.

M. Amusat's process has, however, been very much canvassed, and opposed by other members of the Academy; some stating that it was difficult of performance, and occasionally dangerous; others that it was old, and others that M. Amusat's observations upon the process of tying were not correct.—*Bull. Univ.* C. xviii. 459.

14. *On Metallic Ligatures applied to Arteries.*—Mr. Lerut has lately been led to ascertain the value of a suggestion thrown out some years ago by Dr. Physick, of using leaden ligatures. The idea arose from considering, that in numerous cases, bullets, buck-shot and lead, would remain in contact with almost any tissue of the body,

without producing irritation or unpleasant consequences, and that for an indefinite period.

Mr. Lerut laid bare the right carotid artery of a dog, and after separating it carefully from its accompanying nerve and vein, passed under it a leaden wire, which was then firmly tied. Both ends of the wire were cut off, and the sharp point bent down. The wound was then drawn together by a few stitches and adhesive strips. The animal was left at liberty, and being examined after some days, the stitches were found ulcerated out and the wound open; it had filled up from the bottom with granulations, but the edges were wide apart. With tight dressing it healed entirely, in about ten weeks. A few weeks after, the animal was killed and examined: a small cicatrix existed in the skin; the lead was found in the situation in which it had been placed, by the side of the vein and nerve, perfectly encysted. The artery had been removed entirely, for the space of half an inch. Not the slightest trace of inflammation existed in the neighbouring parts; on the contrary, they appeared perfectly natural. The lead was inclosed in a dense cellular substance, which formed for it a complete cyst.

In four other similar experiments, not the slightest departure from the former appearances occurred. In every case the lead became inclosed in a cyst, and the neighbouring parts remained perfectly healthy and natural. The lead having answered so well, the experiments were continued to ascertain whether that metal was peculiar in this respect, or whether other metals were as innocuous in similar circumstances. Trials with gold, silver, and platinum had exactly the same results; from which Dr. Lerut concludes, that the plan of tying the arteries with lead and the other metals is free from danger, and may be productive of some peculiar advantages.—*Amer. Journ. Med. Sciences.*

15. *Metallic Silver in the Animal Tissue.*—A person who had taken nitrate of silver for epilepsy, was cured, but afterwards died of a diseased liver. His skin had, however, previously acquired the bluish tint now so well known as communicated by the medical use of nitrate of silver, and therefore, after death, the body was examined: it was found that the internal parts, also, had undergone more or less change in colour, as well as the skin. M. Brande undertook the chemical examination of the plexus chorioid and the pancreas, and found both to contain notable quantities of silver.

16. *On the Ergot of Mais, or Indian Corn.*—M. Roulin has written on this subject, and his memoir has been reported upon to the Royal Academy of Sciences. This ergot does not resemble that of rye in its appearance, but produces similar effects. Pigs who feed upon it, lose their hair, and their posterior limbs frequently become paralyzed. With mules the hair falls off, the feet swell, and they frequently lose one or more hoofs; the latter are, gene-

rally, reproduced when the animals are left in pastures. Poultry nourished by such grain, often lay eggs which are without shells; and M. Roulin conjectures, because the ergot causes a convulsive contraction of the oviduct, which expels the egg before there has been time for the calcareous matter to be secreted upon its surface. This disease in the mais is not known, and does not exist in Mexico or Peru; and when grains attacked by it are conveyed beyond Paramos, or the regions of eternal snow in the Cordilleras, they may be used without inconvenience or danger.—*Revue Ency.* xliii. 769.

17. *Poisoning by Strychnia.*—M. Guibourt stated lately to the Academy of Medicine, that having observed a dog in violent convulsions, in consequence of eating one of the compound balls, containing strychnia, or vomica-nut, which the police use to destroy wandering animals, he forcibly made it swallow powdered nut-galls, when the muscular convulsions immediately ceased; ipecacuanha was then given to the animal, but the latter could not vomit; the next day milk was given to it and manna; after which the dog recovered. M. Caventou said, that the infusion of galls was a very effectual opponent to vomiting, and that he had observed it destroy the power of emetic tartar. M. Orfila has already advised the administration of this infusion, in cases of poisoning by opium and salts of morphia.—*Bull. Univ.* C. xvii. 60.

18. *On Vegeto-Alkaline Poisons, and the Neutralization of their Power.*—In a memoir, read by M. Donne to the Academy of Sciences, he states, that morphia, brucia, strychnia, &c., combine with chlorine, iodine, and bromine, to form distinct compounds. These are true chlorides, iodides, and bromides, and may be decomposed by the acids, and the vegeto-alkalies separated. It appears also that these compounds are innocuous in comparison with their bases, and that the compounds of strychnia, when given in doses of 2½ grains, produced no effects on a dog, whilst half a grain of pure strychnia killed a dog of much larger size.

Experiments were then made to ascertain the power of chlorine, iodine, and bromine, as remedies against the poisonous properties of the substances mentioned; and it was found, that if any of these substances were injected into the stomach after strychnia, and in such period of time as had not allowed of sufficient absorption of the latter to produce death, then harm was prevented. In seven experiments of this kind, where doses of one and two grains of veratria had been given, the animals were saved from death by administering tincture of iodine. Once death took place when the antidote was given eight or ten minutes after the poison; and in another case the tincture of bromine given immediately after the administration of a grain of strychnia, failed to save the animal.

When the innocuous compounds of strychnia were decomposed by sulphuric acid and the sulphate formed given to a dog, it killed him in less than an hour. Most of these compounds crystallize regularly, and have particular characters. They are all decomposed by acids. —*Bull. Univ.* C. xviii. p. 289.

19. *Impure Common Salt in France.*—An account has been given to the Academy of Medicine of certain impurities in common salt. M. Cosmeuil, of Rheims, had written to M. Planché, stating, that the salt used at Fere-Champenoise and the neighbourhood had caused serious injury to the inhabitants, and that, in a population of 2400 persons, 400 were ill. These patients were afflicted with violent cholic, accompanied by swelling of the face. M. Cosmeuil examined the salt, and, according to his experiments, found in it bromine, bromide of sodium, iodine, and iodide of potassium.

At the same sitting M. Laugier said, that whilst preparing muriatic acid, in one of his lectures, from common salt, he obtained a large quantity of iodine upon the addition of the sulphuric acid of commerce. M. Orfila also had met with the same circumstance.

Several of the members have been charged with the examination of the salt used in Paris. M. Baruel had met with some containing iodine. His process of detection consisted in diluting a little flour paste with water in a glass, adding to it a drop of chlorine solution and a drop of sulphuric acid, and then putting some of the suspected salt into the mixture. If iodine or its compounds were present, a more or less intense blue or violet colour was produced.

M. Chevallier had examined many specimens of salt which had been seized, but had not found iodine in any of them. Some of the specimens had been adulterated by the admixture of sulphate of soda.—*Bull. Univ.* C. xviii. 472.

20. *Application of Iodine to Scrofula.*—The following is part of a report by MM. Majendie, Serres, and Dumeril, on a mémoire by M. Lugol: " M. le Dr. Lugol has treated 109 scrofulous cases, at the Hospital St. Louis, within 17 months, with iodine alone; at the end of the last year 39 still remained under superintendence, 30 had left the hospital very much improved, 36 had gone away completely cured, and in four cases only did the remedy seem quite inefficacious. The author concludes, from this mass of experience, that iodine should be considered as the most powerful and efficacious remedy in scrofulous cases, since it has constantly arrested the progress of the disease, or at least has exerted a salutary action upon the tuberculous tumours where it has not decidedly produced their cure; and therefore, that in that point of view only its introduction into medicine is one of the most valuable acquisitions the healing art has made in late times." The reporters say they are able to bear witness to the curative power of iodine in these cases, and consider the memoir and exertions of Dr. Lugol as highly useful.—*Revue Ency.* xliii. 767.

21. *On Sulphur found in Gypsum.*—Whilst digging a well at Malvezy, near Narbonne, sulphur was found in soft masses of a clear yellow colour, light, having a scaly structure, adhering to the tongue, and taking a good polish by friction. The gangue of this sulphur was a hard bluish fragile clay, containing traces of bitumen. The sulphur contained about a tenth of bitumen and carbonate of lime. No sulphur was found in the quarry of marly gypsum at Malvezy, but the recent gypsum always possesses a decided odour of sulphuretted hydrogen, and by exposure to air becomes covered with an effloresrence of sulphate of soda. The two strata of blue marl which traverse the quarry are full of sulphate of iron.

The sulphur of Malvezy is found beneath the gypsum; the blue clay containing it is connected with the gypsum, and thus makes part of the lower system of the second fresh-water formation. Sulphur, having the same characters as that of Malvezy, has been found at the old plaster works near Vidilharn.

The particular state of this sulphur shews, that it is the result of a deposite formed by the decomposition of some mineral sulphuretted water. I believe it is certain that this formation of sulphur lies upon the first fresh-water formation (lignite and plastic clay).—M. Tournal.—*Journ. de Pharmacie.*

22. *Falls of the Niagara.*—The American papers state, that part of the great fall has gone down into the chasm below, to the extent of about an acre of the rock, on the Canada side. The curve called the Horse-shoe has been thus much extended. The Table rock is not injured, but immediately above it, in the shoe of the falls, the range has become much more straight. The launch took place at nine o'clock in the evening of the 28th December last.

23. *Thawing Power of a small Stream of Water.*—M. Huber Burnand went to visit the glacier of Grindenwald; and the vault in it, out of which flows the black Lutschine. This vault far surpasses that from which the Aveyron flows, and was estimated at 50 feet in height, and the front of the glacier there at 300 feet. The torrent is of considerable size, but at the time of the visit part of the bottom of the vault was dry. On entering the vault as far as the river would permit, day-light was seen to pass through the roof, and at the place was an enormous tube of ice hanging from above, and ready to fall. The cause of the tube, and the appearance of day-light, very soon appeared; for on leaving the vault and climbing by ladders and steps up the outside of the glacier, so as to arrive at the top and outside of the icy structure, and about 150 feet above the river below, a cavity or crater was seen in the ice, which, upon examination, proved to be a perforation formed by the action of a small stream of water, heated by the sun, and descending from the superior part of the glacier. This was a natural illustration of the fine idea of M. Venetz, applied in the Valley of Bagnes.* The stream

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* *Quarterly Journal of Science,* O.S., vol. xv. p. 390.

was not larger than that of a common fountain, but had so well fuzed and pierced the ice, that it now fell through the roof of the vault into the stream below. The cavity formed, which had probably been enlarged by the current of the air, was a little conical at its orifice, and then vertical and cylindrical apparently to the vault. Its diameter at the mouth was perhaps 24 or 30 feet, but a little way down 12 or 15 feet. It was through this that the light had entered the cavern beneath, and the tube of ice which appeared below was probably a ring of ice detached from the bottom of the well. M. Burnand thinks that the crater will go on enlarging, and that gradually the vault, out of which the river issues, will be separated from the glacier, and stand like an enormous arch alone.

After this he proceeded to a very beautiful natural icy cavern formed in another part of the glacier. The place was perfectly clear, and illuminated by the light which passed through enormous masses of ice; the ice was of a greenish colour. The place was of extreme beauty, and presented a view at each end over the neighbouring country of the most extensive kind. It was concluded that a few weeks of sunshine would destroy the whole.—*Bib. Univ.* xlii. p. 112.

24. *Meteorological Influence of Terrestrial Electricity.*—This subject has been treated of by M. Carlo Matteuci, of Bologna, who endeavours to found certain explanations of natural phenomena upon the supposed accumulation of electricity upon the surface of the earth. He considers that there is accumulation of this power upon particular localities, the electricity itself being developed by evaporation, or other circumstances upon the surface, or by internal chemical action, and when developed, being retained in particular situations by the non-conducting power of the neighbouring earth. This non-conducting power is supposed to depend either upon the particular nature of the ground, or upon its becoming dry by evaporation, and therefore, it is said, it is rather upon elevated and isolated places than upon plains, above rocks than over forests, in summer than in winter, and in the middle of the day than during the night, that those stormy clouds are formed which frequently can only be explained by terrestrial electricity.

One explication furnished by this theory, is considered as ingenious; it applies to those luminous appearances which so frequently occur in the atmosphere during the evenings and nights of summer, and are called heat-lightning. These are attributed to electricity produced and accumulated as already mentioned. After sunset, the vapours which condense, form a conducting stratum near the surface, which serves gradually to re-establish the electric equilibrium between the earth and the atmosphere. It is especially in plains, that these flashes are observed, because the electricity accumulated on high and isolated places escapes rapidly in consequence of their form, lower temperature, and the greater rarety of atmosphere about them.

Sometimes these discharges are supposed to be very powerful; and when the earth and air are very dry, are thought capable of producing earthquakes such as occur after dry seasons; and a very ancient method of avoiding such earthquakes is quoted and approved of, namely, the introduction of long bars of iron into the ground to considerable depths. These, it is supposed, serve to conduct off the accumulated electricity. The *Bibliothèque Universelle* very properly remarks, that it is desirable this accumulation of electricity upon the surface in the manner described, and upon which the whole hypothesis rests, should be proved in a decisive manner.—*Bib. Univ.* xlii. 8.

25. *Petrified Tree in the Isle of Portland.*—This petrified tree was found in the western quarries, nearly half a mile from the sea-shore, and as near as can be judged, about 200 feet above the level of the sea, and about 10 feet below the surface: these petrifactions are found in a sort of bed, or layer of black mould, which in some part appears like burnt wood-ashes, and is from 1 to 2 feet in thickness between the beds of stone. The bed of stone above it is about from 1 foot to nearly 2 feet in thickness, and above that bed, up to the surface, is composed of shingles and slate stone, which is very hard, and is made use of for covering the roofs of houses. The body of the tree is now of an oval form, and is as hard as flint; just above the root it is about 4 feet in circumference, and diminishes to about 2.9; it has the appearance of oak by the grain and knots: it was lying horizontally, and cracked in several pieces, which some think was from the weight on it; but Mr. Beale, a gentleman who has written on this subject, saw it, and affirms it must have been broken by contraction, and that the whole masses of stone must once have been in a fluid state, otherwise a bed of stone could not have been above it.—[Through the kindness of Governor Penn, we have received specimens of the above, which are at the Royal Institution.—Ed.]

26. *Brandy an Antidote to Beer, by* M. Recluz.—During a botanical excursion in the neighbourhood of Lyons, M. Recluz met, in a tavern, a man who was intoxicated from drinking beer, and requested the hostess to give him something to effect his recovery; the latter told M. Recluz that she had nothing but some orange-flower water, of which she put two or three spoonfuls into his mouth. Two minutes afterwards, the drunkenness continuing, M. Recluz himself administered to him some more of this liquor, when he discovered that the bottle, which was labelled orange-flower water, contained only brandy. To repair this mistake, he sent for an emetic, but in the interval the intoxication went off, and the man said he seemed to have awakened from a long and painful dream. M. Recluz has let no opportunity pass of trying the efficacy of this

curious remedy, and always with success. M. Taillet, a physician at Agle, in the course of his practice, has also verified it, and as the subject is not mentioned in any work upon medicine, it has been inserted in the *Annales des Sciences d'Observation.*

27. *Different Methods of preserving Animal and Vegetable Substances designed for Scientific Collections.* By M. Barpay.—The advantages and inconveniences which attend different liquids which have been hitherto employed to preserve anatomical preparations, or individuals of the animal and vegetable kingdom, which enrich our museums, are well known.

Alcohol, which in travelling is not always at hand, involves expenses beyond the reach of private people. This menstruum dissolves the fat and saccharine parts, and the colouring matter, consequently, cannot always meet the views of the collector.

The solution of alum and of nitrate of potass only fulfil their destination imperfectly, and alter the tissues.

Corrosive sublimate, beside the dangers attendant upon the use of it, does not preserve better the entire substance.

Mr. John Davy* has proposed the solution of sulphurous acid gas, which is subsequently filtered, to give transparency to the liquid. This preparation unites, to the lowness of its price, the property of preserving substances for an indefinite time, and of rendering transparent the least compact parts of the organization. In this manner, the author has preserved for three years several anatomical specimens, which are as fresh as at the time of their immersion; the bottles are closed, corked, and luted with wax. The same process, Mr. Davy assures us, may be pursued with regard to vegetable substances. The specimens in pathological anatomy may also be preserved uninjured in the same liquid.

Care must be taken to plunge these substances into the solution before putrefaction has commenced. If it be wished to render the structure of the tissues more apparent, a strong solution must be employed. A weak solution only is required, when anatomical specimens are to be preserved.

This process certainly will not answer for substances of which it is wished to preserve the exact form and colours.

M. Vignal, who has the care of the botanical specimens of the Faculty of Medicine of Paris, has discovered an excellent method of preserving animal substances; it is, to plunge them in water containing an excess of camphor in lumps. As long as the lumps of camphor remain in the water, the specimens are preserved without alteration, even from the contact of air. They would be preserved for an indefinite time in a bottle with a ground glass stopper. M. Vignal, from whom may be expected more ample details on this subject, has kept a fœtus two or three months, or for more than a year, in an open vessel.

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* Transactions of the Medico-Chirurgical Society of Edinburgh, vol. iii. part 1.

These solutions do not appear suited for the preservation of vegetable substances, still less for that of mushrooms.

In 1827, M. F. Leudersdorff* has recommended the use of fat oils mixed with alum or cream of tartar. The alum should be previously separated by strong heat from its water of crystallization. It then penetrates the plants almost as soon as the oil, absorbs all their humidity, and preserves their colours entirely. It is only necessary to steep the plant in this mixture, and then place it between some gray paper for twenty hours, that the oil may penetrate it entirely; afterwards lay it in some dry paper, and in a short time it will be ready for the herbal. When travelling, the plants may be left in a vessel full of oil; the care of drying them being left till you return. As for mushrooms, the author plunges them into mutton suet exposed to a heat of from 42 to 45° R., after having pierced the epidermis of the mushrooms in different points. The suet penetrates into the substance of the mushroom, and what is over is removed when cold; in this way the mushroom retains not only its form, but also its colours, &c.

The chemical composition of mushrooms made me think, for a long time, that an infusion of gall-nuts might be substituted with advantage for pyroligneous acid, which is ordinarily employed to preserve this species of cryptogamous plants. The pyroligneous acid alters the colours, and the consistence of the mushroom. The infusion of gall-nuts with a slight quantity of alcohol, it appeared to me, should obviate these inconveniences, and experience has not contradicted my conjecture. M. Petit, to whom I communicated the fact, has thus preserved, during two months at least, and without any alteration, the most perishable mushrooms. Far from becoming soft, these individuals had acquired in the liquor a greater consistence, and they had perfectly retained their colours. The vessel, however, was simply covered with a cork, and a considerable quantity of mould was upon the surface of the liquid. Care should be taken that the mushroom be entirely immersed in the solution.—*Annales des Sciences d'Observation.*

28. *On the vegetating Wasp of Guadaloupe, by* M. J. B. Ricord-Madianna.—Botanists and entomologists know that particular productions which have been recognised as cryptogamous plants, many of which have been referred to the genus Sphæria, are frequently met with on dead insects, and are preserved in collections; but it has been thought that these plants developed themselves on insects deprived of life. M. Ricord, however, states, that he has observed at Guadaloupe a nest of wasps, the greatest number of which were encumbered with these excrescences. As they quitted the nest, they fell upon the ground, and could not rise again on account of the weight of the plant, which had taken root on some part or

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* Das Austrocknen der Pflanzen, &c. 8vo. pp. 150. Berlin, 1827.

other of their body, particularly on the sternum. Having observed the larvæ contained in the cells, M. Ricord remarked, that they also had this small cryptogamous appendage, but then it was very small. This species appears to be the *Sphæria entomorbiosa* of the English botanists.—*Journal du Pharmacie.*

29. *Artificial Incubation.*—M. Lotz has stated in the *Iris*, that the greatest difficulty in artificial incubation is, to graduate the heat properly; for which purpose he has supplied the following table, the result of his own experiments.

Days.	Pheasants. C.°	Pea Fowl. C.°	Guinea-Fowl. C.°	Partridge. C.°	American Duck. C.°
1	5	5	5	5	5
2	10	10	10	10	10
3	13	13	13	13	13
4	16	16	16	16	16
5	19	19	19	19	19
6	22	22	22	22	22
7 8 9	25	25	25	25	25
10	30	30	30	30	30
11 12 13 14	33	33	33	33	33
15 16 17	38	38	38	38	36
18 19	38	38	38	38.5	36
20	38.5	39	39	39.5	36
21 22 23	38.5	39	39	39.5	37
24	38.5	39	39	39.5	37.5
25		39	39.5		
26		39.5			

30. *Note relative to some blistering Insects, by* M. Farine.—It has been long known that cantharides are not the only insects which possess a vesicatory property, and that it is common to many other genera. From some comparative experiments, M. Farine has ascertained, that the *mylabris cyanescens* is, after the cantharides, the insect which possesses this property in the highest degree; the *mylabris variabilis* follows. The author has also determined, that their action is the stronger when they inhabit warmer places and more exposed to the sun. The male *meloe majalis* is always more

epispartic than the female, and this property is more active in proportion as it is more quickly killed, for if it be kept alive only a few hours it sensibly diminishes. In the same genus the species are more or less vesicatory. The *meloe autumnalis* is less so than the *majalis*, and more than the *meloe reticulata*, and the *meloe tuccia* is infinitely less. The *rissiphorus bimaculatus* and *flabellatus* are without action, while the *rissiphorus subdipterus* is slightly epispartic; the *zonitis prœusta* is innocuous, and the *zonitis punctata* is sensibly active. The period of coupling seems to be that in which this faculty is most developed. The author proposes during this time to collect them, but precautions will then be necessary to prevent the collection made being injurious to the produce of the ensuing year.—*Journal du Pharmacie.*

31. *Ratio of the Births of Males and Females relative to the Age, &c. of the Parents, by* Professor Hofacker.

i. In marriages, where the mother is older than the father, the number of boys (which generally is in the ratio of 101 : 100) is to that of the girls :: 90·6 : 100.

ii. When the parents are of the same age, the ratio of the boys to the girls is 92 : 100.

iii. If the father be from 3 to 6 years older than the mother, 103·4 boys : 100 girls, nearly the ordinary proportion in Europe.

iv. If the father be from 6 to 9 years older than the mother, boys : girls :: 124·7 : 100.

v. If the father be from 9 to 12 or more years older than the mother, boys : girls :: 145·7 : 100.

vi. If the father be 18 years and upwards older than the mother, boys : girls :: 200 : 100.

vii. Young men, from 24 to 36 years of age, with young women from 16 to 26, change the ratio to 116·6 boys : 100 girls.

viii. Young men, with older wives, between 36 and 46 years of age, have boys : girls :: 95·4 : 100.

ix. Men of a middle age (from 36 to 48 years) with young wives, have boys : girls :: 176·9 : 100.

x. Middle aged men (from 36 to 48 years) with wives of a middle age, have boys : girls :: 114·3 : 100.

xi. Middle aged men (from 36 to 48 years) with older wives, have boys : girls :: 109·2 : 100.

xii. Older men (from 48 to 60) with young wives, have given no determinable result, on account of the small number of observations.

xiii. Older men with wives of a middle age, have boys : girls :: 190 : 100.

xiv. Older men with older wives, boys : girls :: 164 : 100.

Many of the above proportions are directly at variance with those of M. Girou de Buzaiengues, and to ascertain the value of them, we should know the data on which they rest, and which have not been given.

32. *On the different Colours of the Eggs of Birds, by* M. Gloger. Verand. der Gesells. Natur. Freunde in Berlin.—It is a remarkable provision of Nature, that birds, whose nests are most exposed, and whose eggs are most open, to the view of their enemies, lay eggs of which the colour is the least distinguishable from that of surrounding objects, so as to deceive the eye of birds or of other plundering animals; while birds, the eggs of which have a bright, decided colour, and are consequently very conspicuous, either conceal their nests in hollows, or only quit their eggs during the night, or begin to sit immediately. It is also to be remarked, that in the species of which the nest is open, and the female brings up the brood without the assistance of the male, these females are generally of a different colour from the male, less conspicuous, and more in harmony with the objects around. The foresight of Nature has, therefore, provided for the preservation of the species of which the nest is altogether exposed, by imparting to the eggs a colour which will not betray them at a distance, while she could, without inconvenience, give the brightest colour under circumstances where the eggs are concealed from view. Or, perhaps, to speak more correctly, numerous birds can deposit their eggs in places accessible to view, because the colour of the eggs makes them be confounded with the surrounding objects, while other birds are obliged to conceal their nests, because the conspicuous colour of the eggs would have attracted their enemies. Let the explanation, however, be what it may, the fact exists, and M. Gloger, who has examined all the birds of Germany, has satisfactorily proved it.

Eggs must be distributed into two series according as their colour is simple or mixed. The simple colours, such as white, blue, green, yellow, are the brightest, and consequently the most dangerous for the eggs.

i. The pure white, the most treacherous of colours, is found among birds which breed in hollow places, like the woodpeckers, the wrynecks, the roller, the merops, the kingfisher, the snow bunting, the robin, the water owzel, the swallow, the martins. It is only among these birds that the eggs are of a remarkable whiteness. The eggs are also white among some species which, like the domestic swallow, certain passeres, the troglodites, &c. construct their nests with such narrow openings, that the eye of their enemies cannot penetrate within. White eggs are also found with birds that quit them only during the night, or at least very late during the day, such as the owls and falcons. Lastly, this colour is found among birds which lay only one or two eggs, and sit immediately after, like the pigeons, the boobies, and the petrels.

ii. As to the bright green or blue colour, it is found to belong to many species which make their nests in hollows, like the starling, the bullfinch, the fly-catcher, &c. In the second place, this colour is common to the egg of birds, the nests of which are constructed with green moss, or placed at least in the midst of grass, but always well concealed, such, for example, as the tomtit, linnet, &c. Lastly, green eggs are met with among many strong birds able to defend themselves against plunderers, like the herons.

iii. A light green colour, verging toward a yellowish tint, is found among the eggs of the many gallinaceæ which lay among the grass, without making a finished nest, which soon disappears beneath the quantity of eggs; like the hoopoe, the perdrix cinereus, the pheasant. The same colour is also remarked among several of the palmipedes, which quit their eggs when they lay them, but which are attentive in watching them, as the swans, the geese, the ducks, the divers, &c. The eggs of certain great birds which make their nests in the open air, but are well able to defend themselves, are a dirty white, as may be observed among the vultures, eagles, storks.

Among the eggs of a mixed colour, they are to be distinguished which have a white ground, and those of which the ground differs from white. The eggs with a white ground are those of the European oriole, the long-tailed tit, the cole-tit, the nut-hatch, the creeper, and the common swallow. Most of the eggs with a white ground are concealed in well-covered nests. The eggs of a mixed colour, and of which the ground is not white, at least of a pure white, are those of the lark, the grasshopper-lark, the yellow hammer, the wagtail, &c.; then the crows, the jays, the thrushes, the quails, &c., with most of the singing birds, the colour of the interior of whose nest harmonizes with that of the eggs*.

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* We have inserted an abstract of M. Gloger's paper, from the attention which his hypothesis seems to have met with from continental naturalists. For ourselves, we have been led to conclude that he is among the number of philosophers who first imagine a system, and then would elaborate facts to support it. The rooks, for example, build a nest particularly exposed on the highest trees; the jackdaws conceal theirs in holes, while the lapwing, woodcock, and snipe lay on the bare ground, and yet the colour of the eggs of all these birds is nearly identical: again, the blackbird and song-thrush are birds of very similar habits; they build in the same places, but the blackbird lays a dull rusty-coloured egg, and the thrush a clear blue one, with a few dark, well-defined spots. The woodpeckers, it is asserted, lay white eggs; they ought according to the theory, but their practices seem very different. The hawks, which are so able and *accustomed* to defend their nests, we should expect to find with pure white eggs, but they are dull-coloured and inconspicuous—the buzzards, the most cowardly among the tribe, have perhaps the most conspicuous eggs of that tribe. The magpie is a strong bird, its eggs well concealed, and the nest fortified; but the colour of this egg is dull, like the rook, woodcock, &c. Two very similar eggs are those of the redstart and hedge-sparrow; the former builds in holes, the latter does not. The cuckoo very commonly selects the nest of the hedge-sparrow, a spotted brown egg among bright blue. Now if we admit that the brightest white eggs are to be found in birds whose nests are the most concealed, as the king-fisher, wryneck, wrens, tits, sparrows, and especially the sand-martin, may we not rather infer that, because the interior of these nests is peculiarly dark, the bright white colour is convenient to the bird, to enable her to distinguish them? At all events, we must regard M. Gloger's hypothesis as ingenious, rather than supported by facts.—Editor.

METEOROLOGICAL DIARY for the Months of September, October, and November, 1829, kept at EARL SPENCER'S Seat at Althorp, in Northamptonshire.

The Thermometer hangs in a North-eastern Aspect, about five feet from the ground, and a foot from the wall.

FOR SEPTEMBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Tuesday ...	1	50	65	29.86	29.83	N	NW
Wednesday.	2	53	66	29.80	29.80	NW	NW
Thursday ..	3	41	63	29.98	29.96	NW	WbN
Friday	4	39	64	29.87	29.80	W	W
Saturday...	5	42	62	29.61	29.40	SE	SE
Sunday	6	47	66	29.30	29.30	W	W
Monday....	7	42	64	29.43	29.44	SSW	SW
Tuesday ..	8	45	64	29.31	29.35	SE	SW
Wednesday.	9	47	65	29.40	29.50	SW	SW
Thursday ..	10	50	67	29.30	29.16	SE	SE
Friday.....	11	49	62	29.32	29.40	WNW	SW
Saturday...	12	43	62½	29.43	29.43	WbS	W
Sunday	13	38	61	29.41	29.18	WbS	SE
Monday....	14	45½	60	29.10	29.26	W	W
Tuesday ...	15	38	60	29.42	29.63	W	W
Wednesday.	16	38	57	29.56	29.48	W	ENE
Thursday ..	17	37	60	29.69	29.60	W	S
Friday	18	41	61	29.15	29.10	SE	N
Saturday...	19	48	58	29.31	29.62	NW	NW
Sunday	20	37	59	29.78	29.70	NW	W
Monday ...	21	46	62	29.67	29.62	W	W
Tuesday ...	22	41	61½	29.67	29.67	SW	SW
Wednesday.	23	45	63	29.67	29.71	W	NW
Thursday ..	24	49	60	29.79	29.87	N	NW
Friday	25	37½	58	29.92	30.03	NW	NE
Saturday...	26	33	61	30.03	29.92	SE	SW
Sunday	27	46	62	29.73	29.65	WSW	W
Monday ...	28	41	56	29.68	29.78	W	W
Tuesday ...	29	40	55	29.82	29.91	W	WNW
Wednesday.	30	43	57	30.02	30.12	NNW	N

FOR OCTOBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Thursday ..	1	37	59	30.12	30.12	NE	NE
Friday	2	46	58	29.98	29.81	E	SE
Saturday...	3	53	59	29.68	29.63	W	W
Sunday	4	44	60½	29.80	29.75	W	W
Monday....	5	51	58	29.47	29.47	WSW	W
Tuesday ...	6	40	53	29.48	29.56	W	WbN
Wednesday.	7	36	45	29.46	29.49	W	NNW
Thursday ..	8	31½	49	29.63	29.90	NW	NW
Friday.....	9	29½	47	30.01	30.18	W	NW
Saturday...	10	31	61	30.21	30.27	SSW	W
Sunday	11	49	60	30.16	30.00	W	WSW
Monday....	12	49	58	29.95	29.80	NW	W
Tuesday ...	13	47	59	29.87	29.60	W	WbS
Wednesday.	14	47	51	29.20	29.76	WNW	N
Thursday ..	15	31	49	30.03	30.13	WbN	NW
Friday.....	16	29½	55	29.93	29.68	S	WSW
Saturday...	17	44	56½	29.80	30.00	W	WNW
Sunday	18	47	61	29.92	29.97	SW	SW
Monday....	19	37	63	29.90	29.86	SW	S
Tuesday ...	20	54	60	29.73	29.64	SW	SW
Wednesday.	21	42	58	29.61	29.50	SW	SSW
Thursday ..	22	44	55	29.41	29.70	W	N
Friday	23	37	50	29.79	29.82	W	WNW
Saturday...	24	24	50	29.82	29.82	WbN	N
Sunday	25	29	53	29.98	30.08	WbS	W
Monday....	26	29	50	30.20	30.20	WbN	ENE
Tuesday ...	27	30	50	30.26	30.20	WNW	WbN
Wednesday.	28	43	50	30.07	30.20	NE	N
Thursday ..	29	30	46	30.20	30.16	W	W
Friday	30	40	54	30.00	29.84	W	WbS
Saturday...	31	41	51	29.73	29.93	NNW	NW

FOR NOVEMBER, 1829.

		Thermometer.		Barometer.		Wind.	
		Lowest.	Highest	Morn.	Eve.	Morn.	Eve.
Sunday....	1	26	41	30.00	30.00	WbN	W
Monday....	2	27	49	30.00	30.03	W	W
Tuesday ...	3	29	50	30.03	29.98	WSW	SW
Wednesday.	4	40	53	29.70	29.48	S	S
Thursday ..	5	33	51	29.71	29.81	W	W
Friday	6	36	51	29.81	29.70	SW	SW
Saturday...	7	31	48	29.70	20.70	W	W
Sunday	8	29	47	29.80	29.81	W	W
Monday ...	9	33	47	29.81	29.88	W	W
Tuesday ...	10	34	50	29.82	29.73	SW	SW
Wednesday.	11	38	52	29.87	29.80	W	SW
Thursday ..	12	41	57	29.70	29.65	SW	SW
Friday.....	13	45	51	29.78	30.00	NE	E
Saturday...	14	38	49	30.00	29.97	E	SE
Sunday	15	37	52	29.75	29.70	W	NE
Monday .. .	16	31	43	29.98	30.10	N	NE
Tuesday ...	17	25	41½	30.20	30.18	NW	NW
Wednesday.	18	30	43	30.18	30.27	NW	N
Thursday ..	19	22	38	30.27	30.27	NE	NW
Friday	20	18	38	30.20	30.19	WNW	W
Saturday...	21	18	39	30.14	30.08	W	SW
Sunday	22	27	40	29.50	29.44	SW	NW
Monday ...	23	33	44	29.50	29.52	N	NE
Tuesday ...	24	31	41	29.60	29.58	E	NE
Wednesday.	25	29	35	29.50	29.52	NE	NE
Thursday ..	26	30	37	29.70	29.75	NE	E
Friday	27	33	40	29.67	29.62	E	EN
Saturday...	28	35	42	29.62	29.50	NE	NE
Sunday	29	37	43	29.65	29.65	SE	SE
Monday....	30	35	42	29.67	29.67	EbS	E

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